THE JOURNAL OF WILDLIFE MANAGEMENT

VOLUME 15 JANUARY, 1951 NUMBER 1

ATOMIC RADIATION HAZARDS FOR FISH

Elmer Higgins

U. S. Fish and Wildlife Service, Washington, D. C.

"The subject of radiation hazards has received considerable publicity lately," said Dr. Bernard S. Wolf in an address before the 18th Annual Meeting of the Greater New York Safety Council, (Wolf, 1948), "but I am afraid no attempt has been made to place this special kind of hazard in its correct perspective with regard to other well-known, but less well-publicized, hazards.

"There has been—and there is—too much unthinking panic when the dangers of radiation are mentioned, even in the minds of those who should know better. At least part of this panic is due to confusion between radiation dangers in case of an atomic war and peacetime radiation hazard control in industry and in the laboratory."

Dr. Wolf lists several of the most worrisome popular beliefs about the dire effects of radiation exposure and then goes through the list one by one disproving the false, debunking the half-true and evaluating the grains of truth in these concepts. He points out that in peacetime, exposure to a fatal amount of radiation can only be due to the grossest kind of blunder. Dangerous intensities of radiation can come only from very special machines or huge amounts of concentrated radioactive materials. Exceedingly careful precautions are taken to prevent exposure to anyone; above all, to the general public.

He emphasizes the oft-repeated thought that just as exposure to excessive amounts of some of the commonest physical, chemical and biological factors in our environment can be harmful, so only may massive doses of radiation produce disastrous effects. There is no unique effect of radiation that cannot be produced by the more familiar agents encountered in our daily lives. Thus, over-exposure to fire or sunlight may be lethal, the effects immediate or delayed. Over-doses of useful drugs such as sleeping pills make headlines. Nicotine from excessive cigarette smoking can shorten lifespan. Fatal amounts of bacteria and viruses can be absorbed without detection by our senses and various chemicals such as the mustard gases can cause genetic mutations which will affect our offspring for countless generations.

On this last point, Dr. Wolf says, "One might hazard the very gross guess that in order to increase the natural mutation rate by a factor of two, every childbearing individual of this generation would have to be exposed to 50 milliroentgens [of radiation] daily, the presently preferred maximum acceptable level in practice, for 10 to 15 years.

Under such conditions, changes [in offspring] might be seen in 150 years."

While all of us in these early years of the Atomic Era are personally concerned with the possible effects upon ourselves and our children of the release of atomic energy, as conservationists, we are also concerned with the possible effects of radiation on fish and wildlife and all of our living resources. It would be natural to transfer our doubts and fears regarding human welfare to the welfare of our animal charges to which so many of us have devoted our lives. It is for this reason that this article is prepared—to allay the fears of unreal or imagined dangers, to assess objectively the few real hazards which exist and to aid in developing an informed public opinion which will assure the application of successful safeguards.

Atomic Wastes as a Source of Radiation Exposure

Leaving aside any consideration of the terrible effects of atomic warfare, it is probable that exposure of our domestic animals and of wildlife to atomic radiation is likely to be through contact with atomic wastes. The fission process yields a great variety of products which are radioactive. The radiations from these by-products, the radioisotopes of a score of elements, are able to penetrate solid matter to a greater or lesser degree. They produce ionization in the molecules of the medium through which they pass and they have definite detrimental effects on living tissues. Since a few of these isotopes will continue to give off radiation for thousands of years, regardless of any chemical or physical treatment, the problem of safe handling and final disposition is apparent. The Atomic Energy Commission, continuing work started by the Manhattan District which produced the first atomic bomb, is keenly aware of the problems of the safe handling of atomic wastes. A great amount of scientific research effort has been directed and is being continued toward their solution.

The four principal methods of radioactive waste disposal used by the Atomic Energy Commission and its subsidiaries are (1) air dilution; (2) water and mud dilution; (3) storage and shipping; and (4) burial. The first two are probably of the greatest concern to the general public. Dr. Morgan says, "Perhaps none of the presently used methods of waste disposal is completely satisfactory for a large scale operation or on a long time basis . . . it is for this reason that studies of new methods of decontamination and safe disposal now under way are believed to be of considerable importance. Most of the health physicists seem to favor some methods of concentrating radioactive waste products and placing them in permanent storage rather than seeking a maximum dilution." (Morgan, 1949).

The reactors of the Atomic Energy Commission, at Oak Ridge and the new Brookhaven Laboratory, are cooled by vast quantities of filtered air which is discharged into the atmosphere through stacks some two hundred to three hundred feet high. The slightly radioactive gases are so diluted by wind currents that they normally reach a tolerance concentration at a distance of less than two hundred feet from the top of the stacks (Beers, 1949). Air dilution appears to be a safe method of disposal for those wastes which are primarily gaseous, and no hazard to

human health or terrestrial wildlife exists under normal practices.

The reactors at Hanford, Washington, are cooled with great quantities of filtered water, pumped from the Columbia River, which temporarily becomes somewhat radioactive. This cooling water and certain other liquid wastes from processes and laboratories, particularly those containing only low levels of radioactivity, are impounded for a sufficient time to permit the radioactivity to decay before being discharged into surface waters. Here a high degree of dilution occurs and in addition, suspended clay and silt absorb or entrap the remaining radioactive material which is thus deposited in the bottom mud. In these undisturbed bottom deposits, further radioactive decay occurs until radiation has virtually disappeared.

More active solid and liquid wastes and solutions are concentrated and stored permanently in tanks underground or buried in guarded areas to prevent unauthorized removal. At Berkeley and at the National Institutes of Health, laboratory wastes are incorporated into concrete and dumped at sea in water more than a thousand fathoms deep.

Water dilution which, next to storage or burial, is the method most used for the disposal of radioactive wastes, is not an ideal solution of the disposal problem. The concentration of radioactivity in the bottom deposits of streams may be temporarily effective in restricting the spread of contamination but floods and stream bed erosion may destroy this stability and carry the contamination further downstream to be redeposited in slack water areas.

While it is true that such fixing in bottom muds is comparable to the hold-up period of liquid wastes in impoundments, permitting radioactive decay to reduce the activity, the constant input of the longer-lived radioisotopes at any point in the stream system must produce an eventual equilibrium between the rate of addition and the rate of removal. Thus, over a period of time, radioactive materials which have reached this equilibrium level of concentration at a given point in the stream move progressively downstream and may eventually reach the ocean. Moreover, although the activity in the water of a stream receiving radioactive wastes may be held, and is normally held, at very low levels so that the water is safe for human consumption and although the activity in the bottom muds in the immediate vicinity of the inflow may remain at only slightly higher levels, aquatic organisms—bacteria, protozoa, algae, higher plants, mollusks, insects, and fish—are known to be able to concentrate within their tissues both stable and radioactive elements, in some cases a thousand fold over their concentration in the environment. Selective absorption of radioactive elements, therefore, permits a greater concentration of activity in the organisms than is found in a unit volume of the water. The exposure of aquatic organisms to radiation would necessarily be more constant and would continue over indefinitely long periods as compared with the casual and more temporary exposure of land animals to airborne contamination. Certainly, aquatic animals cannot escape from an environment which is contaminated.

It is the aim of the Atomic Energy

Commission to perfect the treatment and handling of atomic wastes so as to control more rigidly the activity which enters public waters, and a great deal of progress has been made. Although the Oak Ridge plant of the AEC has released some radioactive wastes into a tributary of the Clinch River under carefully controlled conditions for a number of years, casual inspection indicates that the stream contains an apparently abundant and normal flora and fauna including a good mixed population of warm-water fish. Nevertheless, the presence of even very low levels of radiation as a constant feature of the environment of aquatic organisms in certain streams is a new element in the ecological complex which has not been studied adequately. It is, therefore, important to pursue vigorously an investigation, which is now in the initial stages, on the long-term ecological effects of radiation at levels below human or animal tolerance, (Higgins, 1950).

BIOLOGICAL EFFECTS OF ACUTE EXPOSURE

Research on the biological effects of atomic radiation has been under way for more than a quarter of a century. Stimulated by the construction of the atomic bomb, there has been a great intensification of investigations on the effects of radiation on cells, tissues, organ systems and whole animals and plants, including medical applications. Much of this study has been concerned with the physiology and pathology of irradiation in the usual laboratory animals such as mice, rats, guinea pigs, rabbits and dogs. Moreover, since the objective of many of these studies was the determination of human tolerance limits, the dosages employed were generally high.

Even under the most adverse conditions of waste disposal, such levels of radiation as are used in most laboratory studies are never approached in surface waters as will be shown later. Studies in acute exposure, however, have established tolerance limits for man and other animals, solved many problems in biology and have wide application in medicine.

The resistance of organisms to radiation varies widely between different groups or species. Dr. Shields Warren of the AEC summarizes in a general way these great differences by stating the LD-50 (the dose sufficient to kill 50 per cent of the population receiving it) as determined by various investigators. In general, bacteria are resistant, a killing dose exceeding 200,000 roentgen units (r). Viruses withstand hundreds of thousands of r. In the protozoan, *Paramecium*, the LD-50 is about 300,000 r. In the pupal stage of some insects, the LD-50 is about 150,000 r. In the rat, it is between 825 and 900 r. The guinea pig is one of the most sensitive animals; the LD-50 is about 250 r. In humans, the lethal dose for half of those exposed is about 400 r. Plants in general are far more resistant than are animals (Warren, 1949).

Few studies have been made on the effects of radiation on cold-blooded animals. Bonham and associates (1947) made a brief study of the effects of X-rays (which are comparable to the gamma rays of radioactive isotopes) on several marine plankton organisms with exposures from 1,000 r to 50,000 r. As observed by others, Bonham also found

these one-celled organisms quite resistant, the LD-50 being in the order of about 50,000 r. Bonham (1949) also studied the effects of X-rays on a fresh water snail at doses of 500 to 8,000 r. Immediate death of some adult snails was caused by exposure of 1,000 r and embryonic growth of egg masses so exposed was markedly retarded.

The most extensive study of the effects of penetrating radiation on fish reported in the literature has been carried out at the University of Washington by the Applied Fisheries Laboratory under the direction of Professor Lauren R. Donaldson. Welander (1945) reported the gross anatomical and histopathological effects of X-rays on the growth and development of embryos and larvae of chinook salmon. Eyed eggs were exposed to graduated doses of from 250 r to 10,000 r and observed for 18 weeks. Exposure to 1,000 r or more retarded the development of skin pigment, vascular system, fin rays, interrenal bodies, glomeruli of the kidneys and probably other organs. After 125 days, the length was 10 per cent less and the weight 40 per cent less than in the unirradiated controls. About 13 per cent of the young fish died after 37 to 51 days and 24 per cent 79 to 100 days after exposure probably from injury to the blood-forming tissue and to the gastro-intestinal tract. The most sensitive measure of radiation damage was the number of pycnotic cells (showing damage to the chromosal material of the nucleus) in the hemapoietic tissue of the kidney, injury being detected at exposures as low as 250 r. In all the criteria of damage observed, the effects increased as radiation exposure increased.

Welander and associates (1949) also studied the effects of X-rays on adult rainbow trout at exposures from 50 r to 2,500 r. Exposures of 1,500 r killed 56 per cent in 13 weeks and 87 per cent in 64 weeks. Symptoms of radiation damage seen in fish exposed to 500 r or more, such as hemorrhage, disintegration of tissue, growth of fungus and intestinal damage, were generally proportional to the exposure. Growth was retarded in all fish receiving 750 r or more.

Perhaps the most dramatic demonstration of the effects of penetrating radiation on animals, because some of the effects are visible and involve no mathematical calculations, are reported by Foster and associates (1949). Two of the photographs appearing in the original publication, presented in modified form as Plates 1 and 2 herewith, were furnished by Prof. Donaldson. The results of this experiment are so significant that the authors' summary (pp. 140-142) is quoted here in full:

"One hundred and forty-eight rainbow trout (*Salmo gairdnerii* Richardson) approximately twenty months old and approaching sexual maturity were divided into a control group and seven irradiated groups. Total body irradiation doses of 50 r, 100 r, 500 r, 750 r, 1000 r, 1500 r and 2500 r were administered at a rate of 8.25 r per minute.

"When the fish became mature about three months after irradiation they were spawned, and a total of 115,454 eggs were obtained from 67 females. Although there was an interval of sixty-two days between the time of spawning of the first and last female the data did not show that the X-ray treatments had either delayed or hastened spawning. Some of the fish exposed to 2500 r were so extensively damaged that they died before spawning. Since the parent trout were partially mature at the time they were X-rayed, their developing germ cells also received the radiation. The data did not indicate that germ cells injured by

radiation tended to recover before syngamy.

"The mean mortalities of the eggs obtained from parents subjected to 500 or more r units were significantly greater than that of the eggs from the control parents. Most of the eggs which died during the incubation period contained conspicuously abnormal embryos. The abnormalities could be attributed to deficiencies, improper differentiation of cell masses, disproportionate growth, or combinations of these factors. The deformities which occurred were of almost all conceivable forms, except those of twinning, spinabifida, cyclopians, and the like. Abnormal types of embryos occurred among the progeny of control parents and of parents which had received low doses of radiation which were almost identical with the types which occurred among the progeny of parents receiving large amounts of radiation. However, as the amount of radiation increased, the relative abundance of malformed embryos increased and the degree of development attained decreased. Practically all of the embryos from parents treated with 1500 r and 2500 r were so abnormal that they died before closure of the blastopore. Irradiation of the parent fish thus increased the frequency of occurrence of malformations. Certain types of abnormalities seemed to be more closely associated with irradiation than did others.

"During the fry stage of development the mortality in every group from irradiated parents, even those receiving only 50 r, was significantly higher than the mortality in the control group. After the young fish had started to feed the differences in mortality between the various groups became progressively less. Significantly increased rates of mortality did exist for as long as six months after hatching in groups receiving 500 or more r units.

"The rate of growth of the young trout during their first year of life was also found to be directly affected by the amount of irradiation received by the parent fish. While variations in mortality became less with increasing age of the fish, variations in size became greater. Parents treated with 100 r produced progeny in which growth was slightly impeded, while parents treated with 500 or more r units produced progeny which grew appreciably more slowly than normal."

The experienced fish culturist, on examining plates 1 and 2, will observe at once that the abnormalities of the rainbow trout eggs and larvae from irradiated parents are the same as those encountered in routine hatching operations with normal fish, as the authors point out. The significant point is the increase in the number of abnormal embryos and resulting mortality with the increase in radiation exposure. In the normal unexposed fish, which served as the control in these experiments, mortalities of the eggs ranged from 18 to nearly 40 per cent in various stages of development (A to F, pl. 1). In lots exposed to 50-100 r, mortalities ranged from about 25 to about 60 per cent. In 500 r lots, mortality reached 85 per cent. In 750-1,000 r lots, mortalities reached about 95 per cent and in heavier exposures all eggs died before hatching.

RADIATION LEVELS IN CONTAMINATED STREAMS

As has been pointed out above, such acute exposures to radiation as were commonly used in most laboratory studies are never approached in surface waters. Moreover, methods of treatment, concentration, recovery, fixation and final disposal of radioactive wastes are constantly being improved so that the trend of stream contamination is likely to be downward rather than upward despite increasing manufacture and expanding uses of radioactive materials.

Detailed records of the kinds and amounts of radioactive materials and their levels of activity in streams receiving wastes of AEC installations are, in general, unavailable to the public, but general experience indicates that

the exposure of fish in nature to penetrating radiation is so slight as to provide no basis for alarm. Exact tolerance levels for fish have received little study but a comparison of radiation exposures in certain streams with human tolerance levels is very suggestive.

According to a letter of January 24, 1949 from Dr. K. Z. Morgan, the maximum activity found during 1948 in the Clinch River, which receives some diluted wastes from Oak Ridge, did not exceed 3×10^{-7} microcuries[1] per cubic centimeter. This amount of energy would equal roughly 1/100,000 r of radiation exposure per day. From inspection of the data on the individual isotopes found in this waste, it is apparent that the mean submersion tolerance (for man) is about 3×10^{-3} microcuries per cubic centimeter. In other words, a man could suffer complete submersion in the Clinch River water continuously without radiation damage if the activity were 10,000 times greater than is derived from the atomic wastes the river contains.

For ingestion, human tolerance is lower than for external radiation from immersion because of the possibility of the concentration of activities in certain organs or tissues of the body. Considering the possible effects of the worst beta-emitting isotope in these wastes in the Clinch River, strontium^{90}-yttrium^{90}, the minimal damaging dose for man is 4×10^{-5} microcuries per cubic centimeter according to the AEC. The activity of these isotopes in the Clinch River is therefore only about 1/100 of the minimal value and hence the water is "safe to drink."

The cooling water from the piles at Hanford becomes somewhat radioactive chiefly from the neutron bombardment of traces of sodium which it contains. After hold-up in a settling basin, it is discharged into the river. Because of the short half-life of radioactive sodium (15-hour Na^{24}) and its wide dispersal in living tissues, the permissible concentration here is higher than for many other isotopes. The radioactivity in the Columbia River below Hanford has never exceeded 1×10^{-6} microcuries per cubic centimeter, according to the records of the AEC. This amount is only 1/1000 of the maximum permissible concentration of Na^{24} in drinking water.

The ability of aquatic organisms to concentrate particular ions in their tissues by selective absorption from the surrounding water is well known. A few examples of unusual concentration factors for marine animals range from 13 for magnesium in a mollusk to 2,560,000 for phosphorus in fish. A few others are: 13,000 for silicon in a copepod, 6,900 for fluorine in a mollusk, 6,000 for iron in copepod and fish and 4,300 for copper in a mollusk. (Sverdrup et al, 1942). Thus, aquatic organisms may absorb radioactivity and retain it at much higher levels than occur in the surrounding water.

Radioactive atoms, however, are not absorbed in preference to the stable atoms of the same element. Hence, the percentage of P^{32} atoms among the atoms of P^{31}, the stable isotope found in nature, will be no greater in the tissues of organisms than in the surrounding water. It is for this reason that "isotopic

[1] A microcurie is one millionth of a curie, a unit of measurement equal to the radiation from 1 gram of radium (or more accurately, 37 billion atomic disintegrations per second).

dilution," i. e., the mixing of the radioactive isotope with great quantities of the stable isotope of the same element such as occurs in nature, is so effective is safely disposing of some wastes. In the above case, activity from P^{32} will accumulate in an organism only because of the total amount of phosphorus absorbed. It would seem, then, that radioisotopes of the rarer elements not occurring commonly in surface waters but which are relatively more abundant among the fission products are the ones most likely to play significant roles in the life processes of aquatic organisms. Some of these are only slightly absorbed, some take little part in metabolism, some are rapidly excreted, but some are retained in the animal body for a long time and may be concentrated in a particular organ (e. g., Sr^{90} 25-year half-life, 2.5 Mev. beta emission; a bone-seeker which is excreted very slowly). It is such isotopes that produce serious tissue damage such as necrosis, blood disorders and cancer in the higher experimental animals.

It is the extremely low concentration of radioisotopes in waste waters that prevents these possibilities from becoming serious realities in the Clinch or the Columbia Rivers. Algae in the Columbia River have accumulated radioactivity which approaches 1000 times the level in the water. Occasional organs from suckers living on the algae in the area have shown nearly the same values. But Columbia River salmon rapidly pass through the Hanford area and do not feed on the upstream migration; they therefore do not become measurably radioactive. The AEC reports that the highest dosage received by an organ of a radioactive fish, the liver, measured 0.04 rep[2] per day. This dosage is roughly 80 per cent of the exposure allowed operating plant personnel as a maximum from all radiation and hence cannot be regarded as very serious.

EFFECTS OF LOW-LEVEL EXPOSURE

None of the laboratory studies above reported was concerned with the biological effects of low-level radiation and hence can scarcely indicate effects on aquatic organisms of radioactive waste pollution. It is in the literature on genetics that evidence of damage from low-level radiation is found. Since Muller discovered, in 1927, that X-rays produced mutations in the germ cells and that the natural rate of mutation was accelerated by such treatment, it has been well established that penetrating radiation produces not only breaks and rearrangements in the chromosomes of the cell nucleus but causes mutation of the genes within the chromosomes. Gene mutations and chromosome changes thus produced within the germ cells are inherited according to Mendelian law.

All mutations, of course, are classified as dominant or recessive and as beneficial, deleterious or lethal according to their appearance or effect in subsequent generations. The majority of mutations are recessive, i. e., reveal their presence in the physical structure or physiological function of an organism only if the mutant gene is contributed to the fertilized egg by both parents. The vast majority of recessive mutations are deleterious or lethal in the "doubled"

[2] Rep = roentgen equivalent physical. Unit of measurement of energy absorbed by 1 gram of tissue from particle radiation. Roughly equivalent to r of gamma or X-radiation.

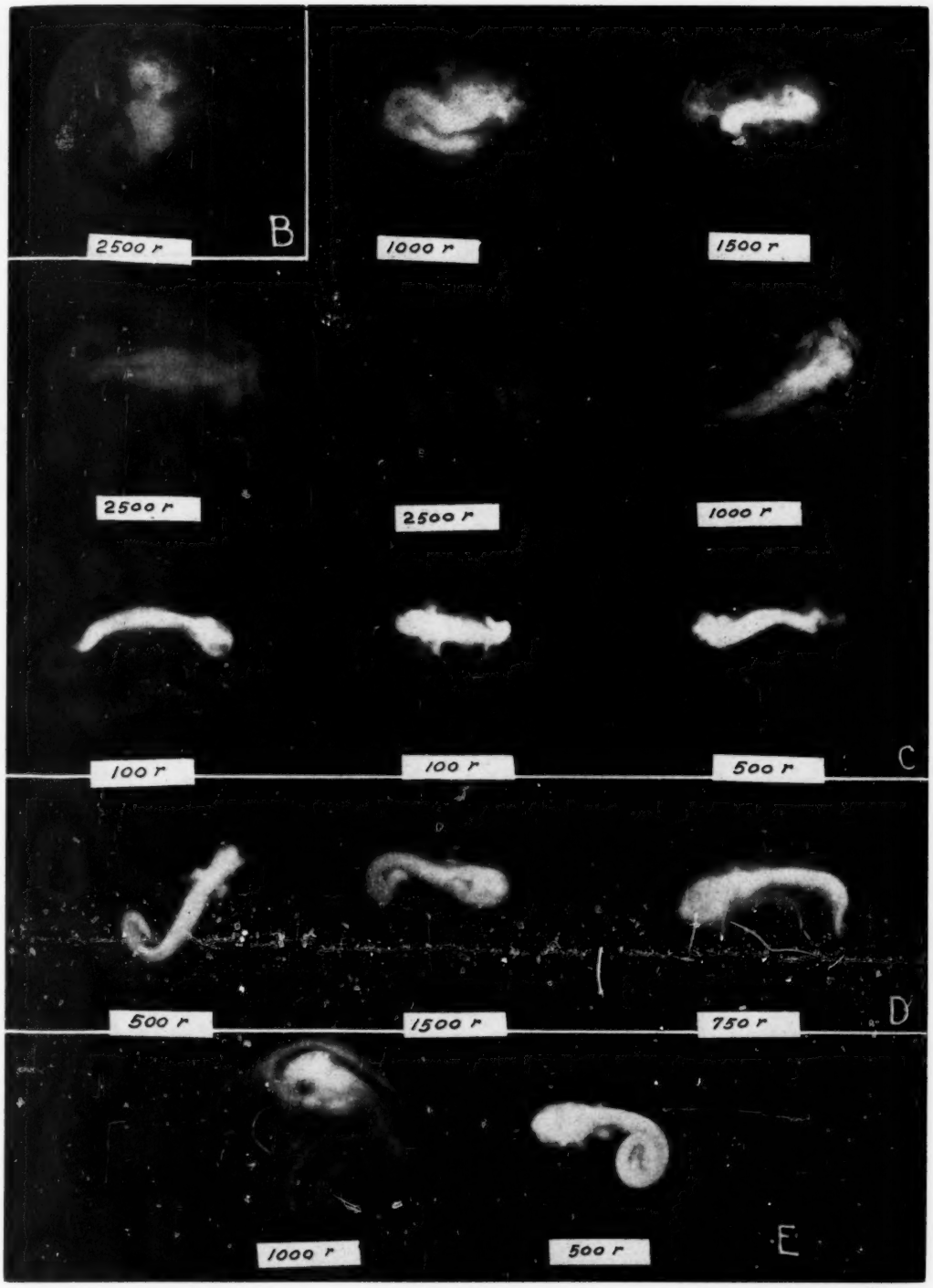

PLATE 1. Abnormal rainbow trout eggs which died after reaching various stages of development. The number of roentgen units (r) of X-ray to which the parents were exposed before spawning is indicated for each egg.

Stages of development: B—embryo recognizable as a multi-cellular blastoderm; C—embryonic axis differentiated from the embryonic shield and the blastoderm had overgrown 1/4 or more of the yolk; D—blastopore closed completely and embryo extended from 1/3 to 1/2 way around the yolk; E—eyes of the embryo had become pigmented and fin rays had appeared. Stages A, eggs which were infertile or in which no embryonic tissue was visible and F, in which the embryo failed to survive the process of hatching, not shown.

After Foster, et al., 1949, figs. 8–21.

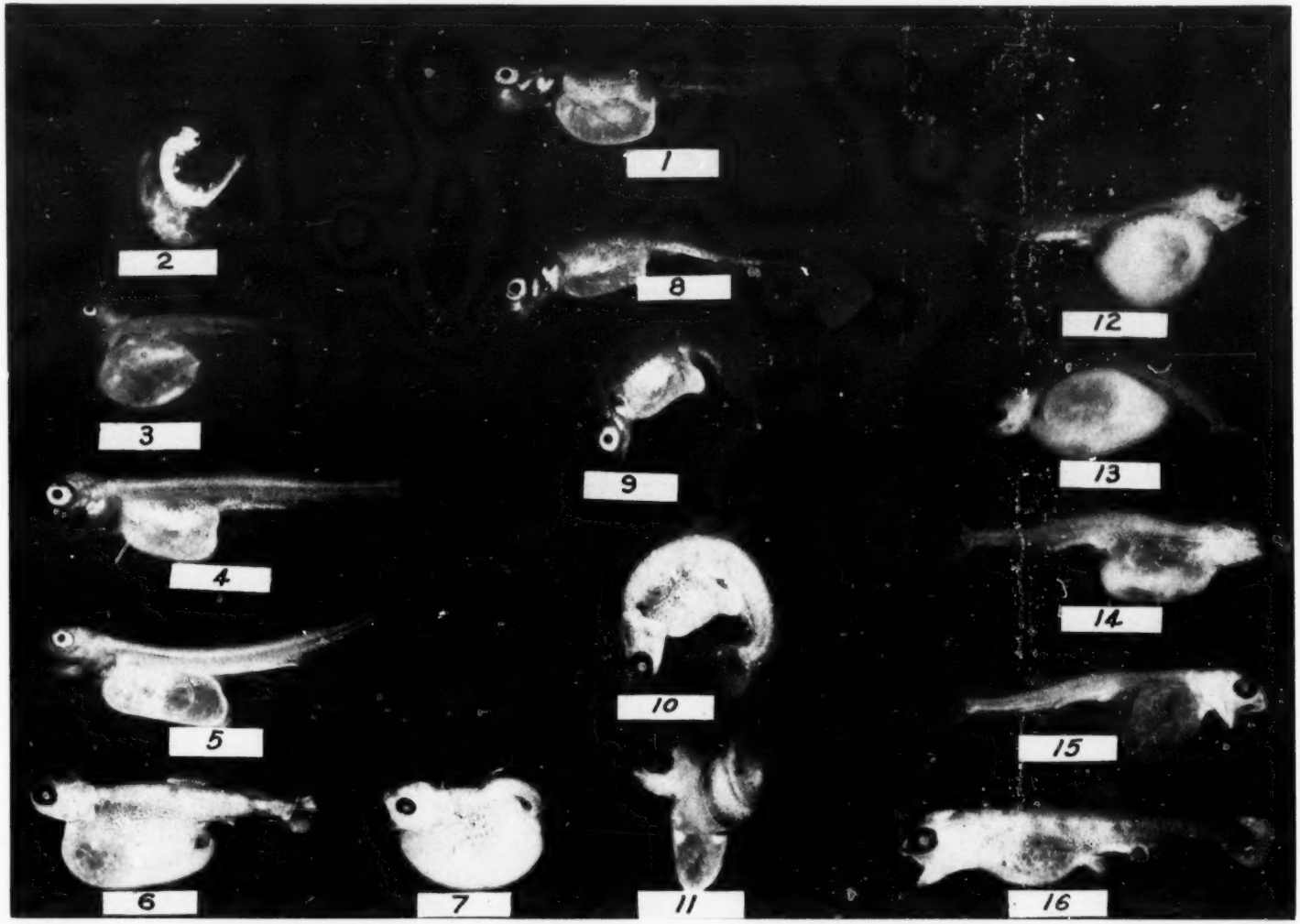

PLATE 2. Common types of abnormal fry found among rainbow trout. Normal development; 1. Deficient development; 2, 3, 4. Weak development; 5. Extravasation or the accumulation of blood or serous fluid; 3. Spinal curvature; 8, 9, 10, 11. Shortened body; 6, 7. Distorted jaw; 15. Defective eyes; 12, 13, 14. Nodule on cranium; 16. After Foster, et al., 1949, figs. 22–37.

or homozygous state. See Dobzhansky (1937) for a review of many aspects of genetics.

In broad outline, the theory of the induction of mutations by radiation holds that a single penetrating particle or quantum of energy passing through or near a gene produces ionization of the atoms forming the gene. This ionization disrupts the chemical bonds among some of the million or so atoms in the gene molecule thus changing its chemical structure and its subsequent effect on the course of development of cells and tissues. Similarly, the passage of ionizing particles or quanta through a chromosome breaks the chromosome thread, the fragments of which may rejoin, may join in new arrangements, or may fail to join and thus be lost in later cell divisions with consequent effects on the descendants of the cell. If produced in a reproductive cell, these changes may be inherited in subsequent generations. See Lea (1947) for a full review.

As a result of exhaustive experiments on the genetics of the fruit fly, of mice and of many plants, it is held that the number of induced mutations bears a linear relationship to the total amount of radiation absorbed by the sensitive volume of the cell and is independent of either the duration or the intensity of exposure. Consequently, a long exposure to low-level radiation would have the same genetic effect as shorter exposure to a higher level. Experiments of Spencer and Stern (1948) on the fruit fly show that the percentage of sperm containing a sex-linked lethal mutation is increased about .002 per r of radiation exposure and that 50 r exposure is required to double the natural mutation rate.

Spencer and Stern (l.c.) conclude their exhaustive study of the validity of the linear relationship between radiation exposure and mutation frequency with the statement (p. 64): ". . . for radiation with X-rays, dosages as low as 25 r produce mutations as drastic in their effects and in the same proportion to the dosage as do exposures to high dosages. If an extrapolation is permissible, one may assume that there exists no tolerance dose below which mutations are not induced. The total effect of X-rays on the induction of point mutations in a fruit-fly population would depend, then, on the mean value of r-units to which the individuals of the population are exposed. In other words, the total effect on the germplasm of a population of 100,000 individuals would be as great if each individual received 1 r at the beginning of the reproductive period as it would be if 1,000 individuals each received 100 r and the others none . . . . It should also be kept in mind that for all practical considerations mutation of the germplasm is an irreversible process. The new mutation, once it occurs, cannot be cured or alleviated. As most transmitted mutations, however deleterious their effects, are recessive, the importance of the induction of a mutation is not to be reckoned in terms of its effect on one individual, but rather in respect to its possible effect through transmission to many individuals over a large time scale . . ."

The classical hit theory of induction of mutations, particularly the linear relation between dosage at low levels and mutation rate, has been questioned by Caspari and Stern (1948), who found no significant difference in mu-

PLATE 2. Common types of abnormal fry found among rainbow trout. Normal development; 1. Deficient development; 2, 3, 4. Weak development; 5. Extravasation or the accumulation of blood or serous fluid; 3. Spinal curvature; 8, 9, 10, 11. Shortened body; 6, 7. Distorted jaw; 15. Defective eyes; 12, 13, 14. Nodule on cranium; 16. After Foster, et al., 1949, figs. 22-37.

tation rates in the sperm of the fruit fly between controls and experimentals exposed to 2.5 r per day for 21 days. Uphoff and Stern (1949), however, after further tests, concluded that low-level radiation does produce mutations in fruit-fly sperm and that the apparent inconsistencies of previous results were due to different experimental techniques and errors in sampling.

At any rate, there can be no doubt that penetrating radiation produces changes in the germ-plasm of a population, and that these changes, however small, will persist and will produce positive effects.

This does not mean, of course, that radiation will produce freaks such as two-headed monsters in the progeny for such abnormalities are produced by other means. Nor does it mean that other obvious changes will be apparent in the early progeny of irradiated parents. In the many thousands of genes in the eggs and sperm of vertebrate animals, natural mutations occur at a very low rate from causes not yet understood. The loss or change of a single gene, therefore, may be inconsequential as concerns a single individual. Hence, we must not expect the sudden appearance of strange new kinds of fish in our waters even if they are contaminated with radioactive wastes nor need we fear the sudden disappearance of a population through the production and accumulation of lethal genes by mutation from such irradiation. The natural mutation rate is so low, the number of induced mutations from low-level radiation is so small and the apparent effects are so long delayed that changes in the fish population, real as they are, may be too obscure to be detected in the space of a few years by present methods of observation.

In his article on radiation damage to genetic material, Muller (1950) gave an example, based on a number of assumptions, of the genetic effect on a population exposed to a moderate amount of radiation. He said, in effect, (page 54), that if a group of a thousand first-generation offspring had inherited 150 deleterious or lethal mutations induced by radiation and if these offspring and their descendants continued breeding in the ordinary way, some 150 genetic deaths would occur in the descendent population over a long period of time. In addition there would probably be, in intermediate generations, a number of individuals several times as great as this 150 who are not killed by the induced effects but whose health and vigor would be materially hampered or reduced. This would occur in addition to (1) the effects of the many spontaneous mutations accumulated from the near and distant past which simultaneously take their toll and (2) the causes of death and morbidity of environmental origin. These other factors would hide from our view the effects of the mutations induced in the single generation in question.

Muller concluded that, for many generations, there will surely be no perceptible effect since each "layer" of mutations induced in a single generation will be spread out too thinly and remotely to be observed among the accumulated spontaneous mutations. He points out, however, that, under continued irradiation, gradually the "layers" will become superimposed resulting

in new equilibrium values for the frequency of mutant genes, generation by generation, corresponding with the new rate of mutation. Since each deleterious or lethal mutation eventually causes one genetic death, the frequency of these deaths will slowly rise.

There is no satisfactory way of clearly distinguishing between genetic deaths and deaths resulting from environmental causes, but the latter must far out-number the former. The genetic deaths resulting from mutation, even though they may reduce the population or change the species composition, cannot be regarded as defects in the web of life, for under natural selection the population is thus purged of inherited weaknesses. In any case, these genetic changes may develop with the glacier-like slowness of normal evolutionary processes and may escape detection by any means except exacting laboratory tests.

SUMMARY AND CONCLUSIONS

Aquatic animals are more likely to be exposed to irradiation, through contact with atomic wastes in a few streams, and thus are more subject to damage than are terrestrial animals. Although there is a possibility, through selective absorption, of the concentration of activity in certain tissues of aquatic organisms with attendant localized damage, there is as yet no evidence of pathological conditions caused by radiation in organisms so exposed under natural conditions. Few specific observations of this sort have been made but, in view of the fact that radiation levels in contaminated streams are far lower than human tolerance limits or the levels used in laboratory experiments to demonstrate or measure the injurious effects of radiation on living tissues, no such direct injury should be expected.

Much emphasis has been placed by geneticists on the possible damage to the germ-plasm of populations exposed to radiation. Based on extensive experiments with insects, plants and mammals, there is a theoretical basis for the belief that genetic change is the most certain, but by no means the most imminent, result of low exposure. Such change in inheritance will be so small and so long delayed as to have little significance from the conservation point of view.

These long-range judgments, based on a wide variety of evidence, are not specifically supported by direct experiment. Time has not yet permitted tests of their validity. Until the obscure effects of long-continued low-level radiation exposure are thoroughly understood, research in this field should be continued and expanded. We must assess any dangers, small as they may be, to our living resources not only to measure their real importance but to define the practical and economic limits of waste treatment and to devise and apply such safeguards as may be needed.

LITERATURE CITED

BEERS, NORMAN R., 1949. Stack Meteorology and Atmospheric Disposal of Radioactive Wastes. Nucleonics, 4(4): 28–38.

BONHAM, KELSHAW, ALLYN H. SEYMOUR, LAUREN R. DONALDSON and ARTHUR D. WELANDER, 1947. Lethal Effect of X-rays on Four Marine Microplankton Organisms. Atomic Energy Commission, MDDC-1158, 13 pp. (Mimeographed).

BONHAM, KELSHAW, 1949. Effects of X-rays on the Fresh-water Snail, *Radix japonica*.

Atomic Energy Commission, UWFL-21, 30 pp. (Mimeographed).

CASPARI, ERNEST and CURT STERN, 1948. The Influence of Chronic Radiation with Gamma-rays at Low Dosages on the Mutation Rate in *Drosophila melanogaster*. Genetics, **33**: 75–95.

DOBZHANSKY, T., 1937. Genetics and the Origin of Species, Columbia University Press, New York, 364 pp.

FOSTER, RICHARD F., LAUREN R. DONALDSON, ARTHUR D. WELANDER, KELSHAW BONHAM, and ALLEN H. SEYMOUR, 1949. The Effect on Embryos and Young of Rainbow Trout from Exposing the Parent Fish to X-rays. Growth, **13**: 119–142.

HIGGINS, ELMER, 1950. Radioactive Wastes and their Significance in Stream Ecology. Transactions of the American Fisheries Society, **79**: 217–232.

LEA, D. E., 1947. Actions of Radiations on Living Cells. MacMillan Company, New York, 402 pp.

MORGAN, K. Z., 1949. New Public Health Problems in the Atomic Era. *Chapter in* Public Health in the World Today, edited by J. S. Simons, Harvard University Press, Cambridge, Massachusetts, pp. 292–316.

MULLER, H. J., 1950. Radiation Damage to the Genetic Material. American Scientist, **38**(1): 33–59, 126.

SPENCER, WARREN P. and CURT STERN, 1948. Experiments to Test the Validity of the Linear r-dose/Mutation Frequency Relation in Drosophila at Low Dosage. Genetics, **33**: 43–74.

SVERDRUP, H. U., M. W. JOHNSON and R. H. FLEMING, 1942. The Oceans, their Physics, Chemistry and General Biology. Prentice-Hall, New York. 1087 pp.

UPHOFF, D. E. and CURT STERN, 1949. The Genetic Effects of Low Intensity Irradiation Science, **109**(2842): 609–610.

WARREN, SHIELDS, 1949. Health Problems in Atomic Energy Activities. *In* Seminar on the Disposal of Radioactive Wastes. Atomic Energy Commission, Press Supplement, No. 154, Jan. 30 (Mimeographed).

WELANDER, ARTHUR D., 1945. Studies of the Effects of Roentgen Rays on the Growth and Development of the Embryos and Larvae of the Chinook Salmon (*Onchorhynchus tschwaytscha*). Atomic Energy Commission, UWFL–2, 131 pp.

WELANDER, ARTHUR D., LAUREN R. DONALDSON, RICHARD F. FOSTER, KELSHAW BONHAM, ALLYN H. SEYMOUR and FRANK G. LOWMAN, 1949. The Effects of Roentgen Rays on Adult Rainbow Trout. Atomic Energy Commission, UWFL–17 (AECU–188), 17 pp. (Mimeographed).

WOLF, BERNARD S., 1948. Medical Aspects of Radiation Safety. Nucleonics, **3**(4): 25–29.

Accepted for publication March 22, 1950.

CONFIDENCE LIMITS FOR THE PETERSEN OR LINCOLN INDEX USED IN ANIMAL POPULATION STUDIES

*Lowell Adams**

U. S. Fish and Wildlife Service, Missoula, Montana.

Usually wild animals in their natural habitats are not easily counted. Several "census" techniques have been designed to overcome this difficulty. One of the most useful of these is known to fishery biologists as the Petersen Method and to the land-animal biologists as the Lincoln Index. This technique consists of introducing a known number of marked animals of the kind being inventoried into the habitat with the population. The introduced animals are marked so that they may be distinguished from the rest of the population. Samples are then drawn from the population and total numbers are estimated on the basis of the ratio of marked to unmarked animals in the sample. Let x equal the number of marked animals in the sample of n animals. Also let m equal the number of animals that were originally marked. Then the estimated total population, T, would be:

$$T : m :: n : x$$

or

$$T = m\frac{n}{x}$$

$$= \frac{m}{(x/n)}$$

One purpose of this paper is to present a method for statistical refinement of the census technique. This refinement, which provides confidence limits for the population index, was developed in statistical theory several years ago, but it has been little used in the present application. A second objective of this paper is to reproduce graphs of the confidence limits so they may be readily available as working tools for biologists who may wish to determine limits for population indexes.

The ratio index to animal populations was first described by C. G. J. Petersen in 1896 (Ricker, 1948) for fish populations. F. C. Lincoln (1930) described its use in estimating waterfowl populations. For further details of its use, reference is made to the excellent review of the subject by W. E. Ricker (1948). This publication is of great value to both fisheries and land-animal biologists. Of special interest is the list of conditions which must hold if the above equation is to be valid:

(1) The marked animals must suffer the same natural mortality as the unmarked ones.

(2) The marked animals must not lose their marks.

(3) The marked animals must be as subject to sampling as the unmarked ones.

(4) The marked animals must become randomly mixed with the unmarked ones, *or* the distribution of sampling effort must be proportional to the number of animals in different parts of the habitat being studied.

*In cooperation with the Northern Rocky Mountain Forest and Range Experiment Station.

(5) All marked animals must be recognized and reported on recovery.

(6) There can be only a negligible amount of recruitment to the population being sampled during the sampling period.

Jackson (1939) has developed a technique for measuring birth, death, and migration onto and off the area studied during the study period. This last condition is therefore unnecessary if Jackson's method is used.

Even under the above conditions it is apparent, according to the laws of chance, that the ratio of marked to unmarked animals in the sample will not always be the same as the ratio of marked to unmarked animals in the population. In fact the two ratios may *seldom* be the same. For example, in selecting a sample of 50 animals from a population of 500, of which 100 are marked, it is theoretically possible to get a sample in which, instead of the

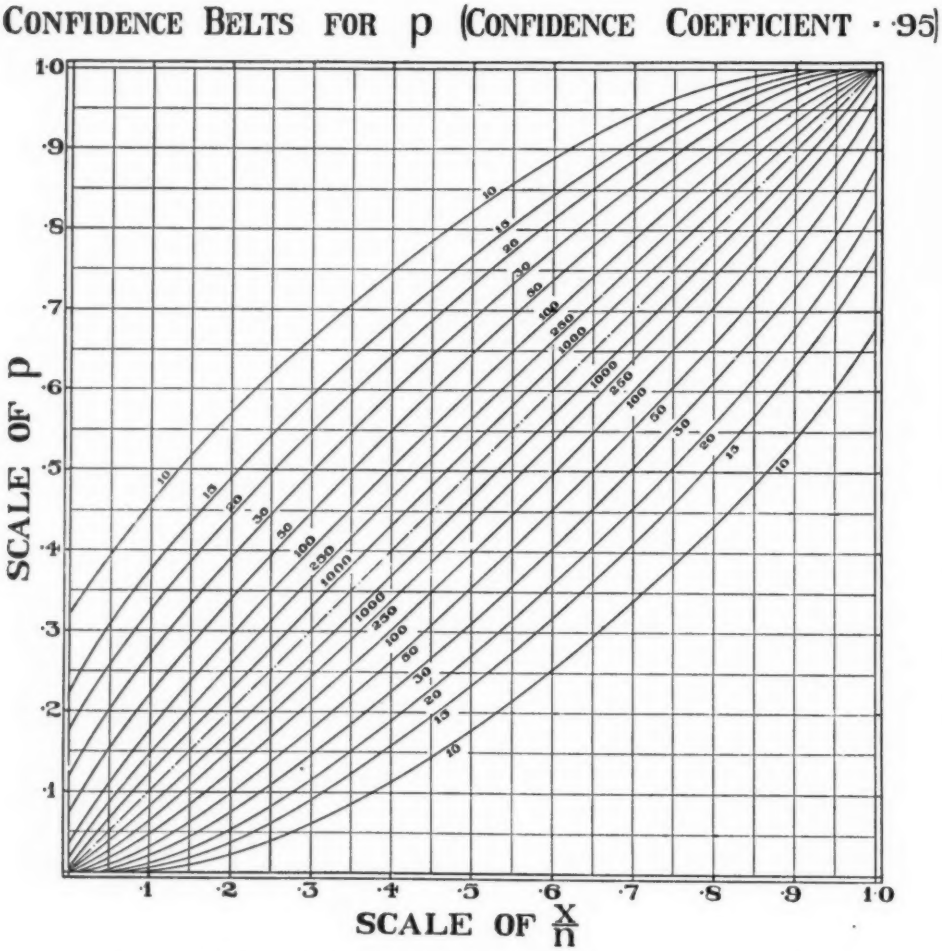

FIG. 1. Confidence limits for the binomial distribution at the 95 per cent level.

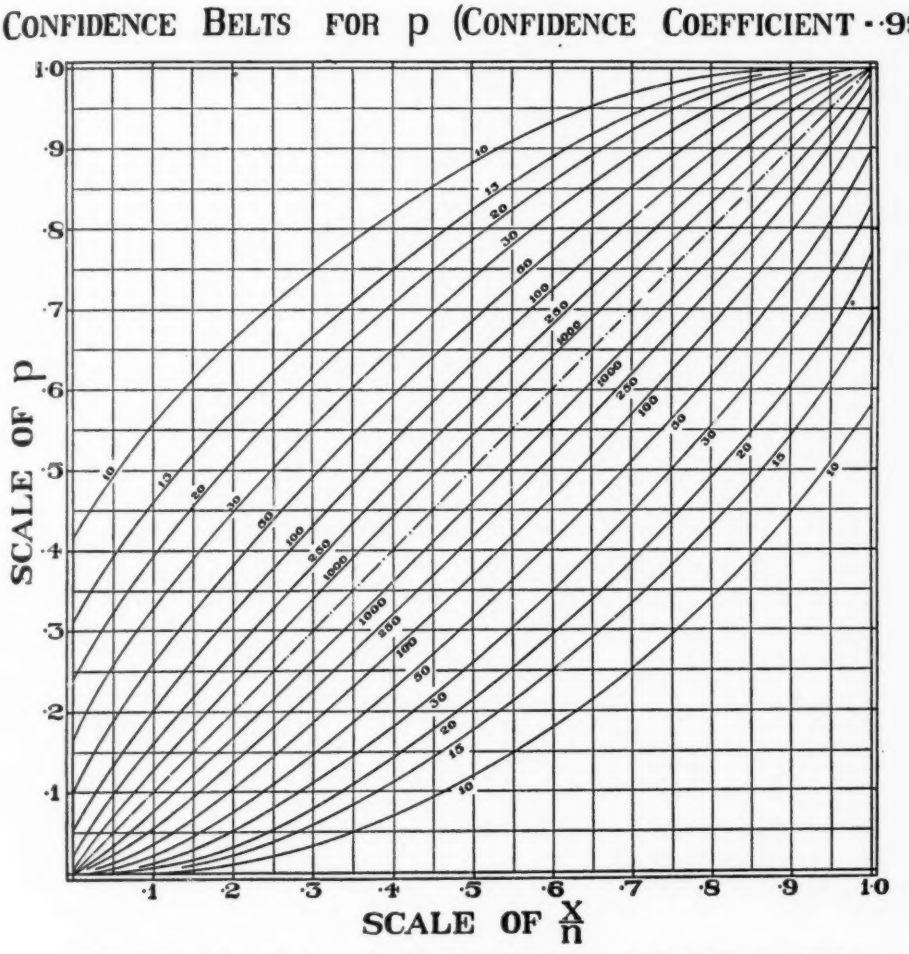

FIG. 2. Confidence limits for the binomial distribution at the 99 per cent level.

population ratio of 1 marked to 5 unmarked animals, all animals are marked, or in which none are marked. Or, one could get a sample of 25 marked and 25 unmarked animals. These theoretical possibilities have little likelihood of happening, but they illustrate the possibilities for error. There are all degrees of possible errors from getting all marked animals to getting all unmarked animals in the sample. But there is a tendency to get a sample ratio which approaches more or less closely the true population ratio. Of all possible sample ratios, the one most likely to occur in the sample is the true ratio of the population. But ratios close to that of the true population are nearly as likely to occur as the true ratio. The farther one gets from the true ratio, the less likely is the occurrence in the sample.

Possibilities for sampling error can be decreased by increasing the size of

the sample. As the sample size approaches the population size, chances for error become smaller. When the point is reached where the sample includes the entire population, there is no error. From this relationship one can determine the confidence limits for a sample of a given size. The statistical computations for this are quite involved, but fortunately they have already been made (Clopper and Pearson, 1934). Confidence limits are presented in graphic form for the 95% and 99% levels (Figs. 1 and 2).[1]

The scale across the bottom of the graph (scale of $\frac{x}{n}$) represents the ratio of marked animals in the sample (x) to the total number of animals in the sample (n). The vertical scale (scale of p) represents the population ratio. The use of the graph is best illustrated by an example.

One hundred marked animals are released in a population. A later sample of 250 animals from the population includes 25 marked ones.

$$\frac{x}{n} = \frac{25}{250} = 0.1$$

The best estimate of the population number in this case would be $\frac{100}{0.1} = 1000$. But since the sample ratio may have varied from the true population ratio, it is desirable to determine within what limits the variation may have occurred. Referring to the scale of $\frac{x}{n}$ in Fig. 1, read up the line .1 to the intersection with the curve marked 250. Thence, reading across to the scale of p, you come qut at approximately .07. Now go back to the .1 vertical line and follow it up to the second 250 curve. Read across as before. The value here is approximately .14. The two readings obtained from the graph indicate that the chances are 19 to 1 (95 to 5) that the true population ratio lies between .07 and .14, and that the true population is somewhere between $\frac{100}{.07} = 1429$ and $\frac{100}{.14} = 714$.

If greater assurance is desired, the .99 confidence coefficient (Fig. 2) may be used. Using the same example as above the ratio confidence limits here are .06 and .16. These indicate population confidence limits of $\frac{100}{.06} = 1666$ and $\frac{100}{.16} = 625$. This indicates chances are 99 to 1 that the true population lies within these limits. Interpolations are easily made for sample sizes and ratios which fall between the lines of the graphs.

A more realistic example of the use of the graphs is provided by using data from actual wildlife investigations. Green and Evans (1940, their Table 5) have presented data for the Lincoln Index in relation to snowshoe hares. The number of marked animals in the sample (x) is line 2 in their table. The total sample (n) is line 2 plus line 3. The number banded in the pre-census period (m) is shown in line 1. The total population (T) is in line 7. (In this case we are calculating the total population directly from the marked animal ratio, instead of calculating the number of unmarked animals as Green and Evans did.) For 1932-33 the equation is

[1] Permission to reproduce these graphs was kindly granted by Prof. E. S. Pearson, Editor, Biometrika.

$$T = \frac{m}{(x/n)}$$

$$= \frac{201}{37/109}$$

$$= 591$$

Here $\frac{x}{n} = .34$. Entering Figure 1 at $\frac{x}{n} = .34$ with a sample of 109, we find the p scale readings at .44 and .25. Therefore the true population of hares will lie between $\frac{201}{.44} = 457$ and $\frac{201}{.25} = 804$, with 95 per cent probability. Green and Evans (their Table 7), using mathematical computations, obtained population confidence limits of 466 and 811 at the 95 per cent level.

Further comparisons with the "Population Range" of Green and Evans are shown in Table 1. For large samples and samples which approach a ratio of 0.5, the mathematical computations are fairly accurate. But for smaller samples and samples which approach ratios of 0.0 and 1.0, the Clopper and Pearson graphs are considerably more accurate.

TABLE 1.—CONFIDENCE LIMITS FOR HARE DATA COMPARING RESULTS FROM MATHEMATICAL COMPUTATIONS WITH THOSE FROM THE CLOPPER AND PEARSON GRAPHS

Year	Population Confidence Limits	
	Green and Evans	Clopper and Pearson (Fig. 1)
1933	466–811	457–804
1934	535–697	532–713
1935	546–793	544–817
1936	282–564	271–573
1937	138–360	135–385
1938	51– 94	51–112
1939	83–166	85–172

The graphs of confidence limits are for use when the sample is a relatively small part of the population. If the graphs are used when the sample is larger they will give limits that are too broad (13 per cent error when sample/population = 0.25; 29 per cent error when sample/population = 0.5). The error is of a conservative nature, however. That is, significance is never indicated when it does not exist. The same restrictions apply to the use of the Poisson curves described later.

When the value of $\frac{x}{n}$ is very small (less than approximately 0.05) the confidence limits presented in the Clopper and Pearson graphs are difficult to read. Such small values of $\frac{x}{n}$ frequently occur in wildlife work, usually when n is rather large. Provided n is greater than about 25, confidence limits for the Poisson distribution can be used. These are shown in figures 3 and 4 (adapted from Ricker, 1937). These figures are entered on the abcissa using the value of x instead of $\frac{x}{n}$ as in the Clopper and Pearson graphs. The limits to be read on the ordinate scale are the confidence limits of x. Ratio values of $\frac{x}{n}$ and p are obtained by dividing by n, the number in the sample.

As an example, in a sample of 2,000 salmon, 6 tagged salmon were found. For $x = 6$ at the 95 per cent confidence level, the limits between which this figure may lie are 1.5 and 13. Dividing these by 2,000, a likely range for the fraction of tagged salmon in the population is from 0.00075 to 0.0065.

The graphs of confidence limits may be used for other population ratios

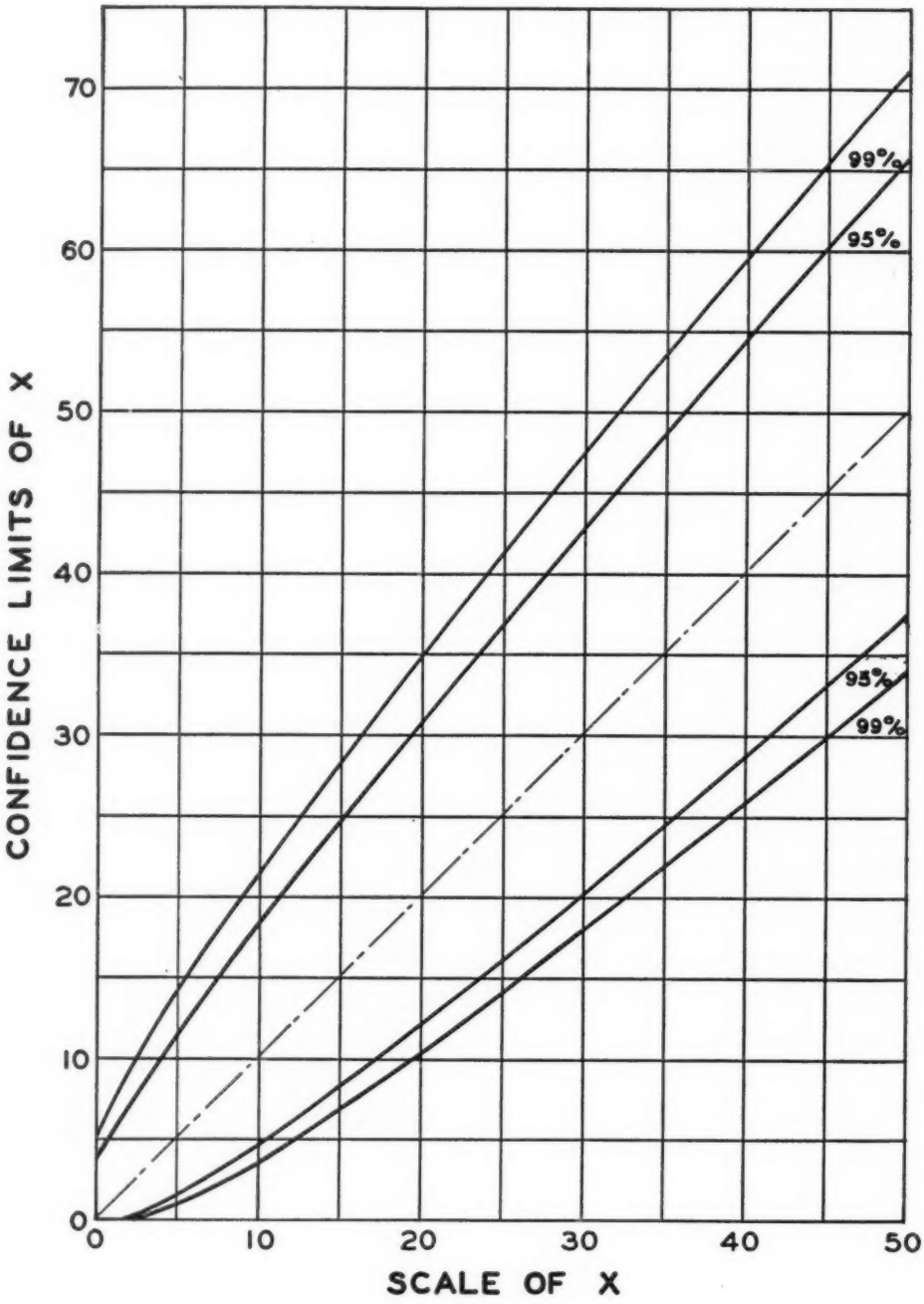

FIG. 3. Confidence limits for the Poisson distribution at the 95 per cent and 99 per cent levels.

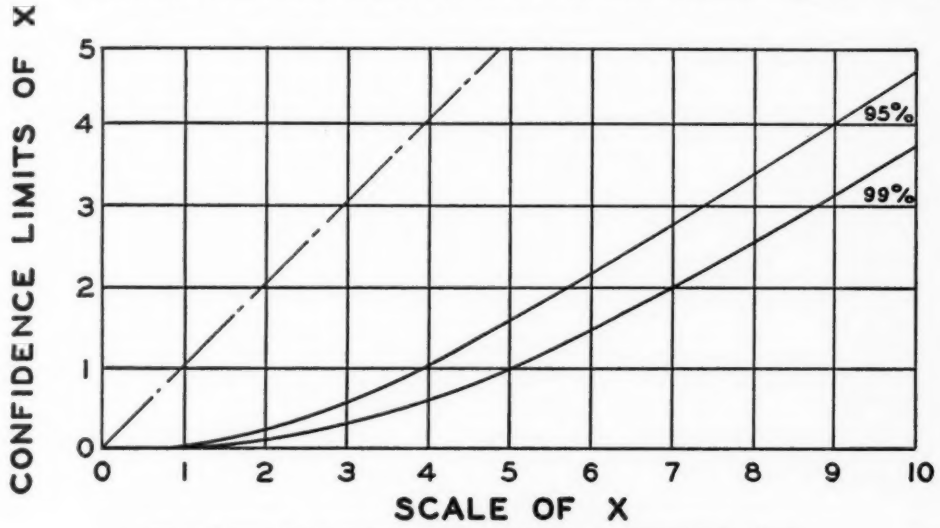

FIG. 4. Lower left corner of Figure 3 enlarged for convenience of use.

besides the Petersen or Lincoln Index. Confidence limits for sex ratios, age ratios, size ratios, hunter and fisherman success ratios and many others may be obtained from the graphs. As an example, if a deer herd exhibits an adult-fawn ratio of 3 to 1 (= .33) in a sample of 300, the population ratio will lie between approximately .28 and .40 confidence limits at the .95 level.

Several persons deserve credit for their assistance in preparing this report. Dr. Lucille Stickel, Biologist, Patuxent Research Refuge, Fish and Wildlife Service, suggested the problem and gave helpful suggestions during the preparation of the report. Mr. James G. Osborne, Biometrician, Division of Forest Economics, U. S. Forest Service, called my attention to the Clopper and Pearson graphs and suggested their application to the Petersen or Lincoln Index. Dr. N. W. Hosley, recently In Charge, Section on Wildlife Research on Public Lands, U. S. Fish and Wildlife Service, encouraged the work and assisted with the manuscript. Professor William E. Ricker, Indiana University, reviewed the manuscript and offered many helpful suggestions.

LITERATURE CITED

CLOPPER, C. J. and E. S. PEARSON. 1934. The use of confidence or fiducial limits illustrated in the case of the binomial. Biometrika, **26**: 404–413.

GREEN, R. G. and C. A. EVANS. 1940. Studies on a population cycle of snowshoe hares on the Lake Alexander area. I. Gross annual censuses, 1932–1939. Jour. Wildlife Management, **4**: 220–238.

JACKSON, C. H. N. 1939. The analysis of an animal population. Jour. Animal Ecol., **8**: 238–246.

LINCOLN, F. C. 1930. Calculating waterfowl abundance on the basis of banding returns. U. S. Dep't. of Agriculture Circular No. 118, 4 pp.

RICKER, WILLIAM E. 1937. The concept of confidence or fiducial limits applied to the Poisson frequency distribution. Jour. Amer. Statistical Ass'n., **32**: 349–356.

RICKER, WILLIAM E. 1948. Methods of estimating vital statistics of fish populations. Indiana University Publication, Science Series No. 15, 101 pp.

Accepted for publication December 30, 1949.

EFFECTS ON WILDLIFE OF DDT USED FOR CONTROL OF DUTCH ELM DISEASE

Allen H. Benton[1]

Introduction

Dutch elm disease, the causal agent of which is a fungus, *Ceratostomella ulmi*, is spread from tree to tree by certain bark beetles of the family Scolytidae. In eastern United States two important carriers are the native elm bark beetle (*Hylurgopinus rufipes*) and the smaller European elm bark beetle (*Scolytus multistriatus*). In the area of this study the latter is of greater importance.

Control of the disease involves control of the beetle vectors which introduce spores of the fungus at the time of feeding. Spraying programs are based on the application of sufficient DDT to elm trees to kill the bark beetles before feeding takes place. Since adult beetles emerge and fly to new trees during several spring and summer months, it is necessary to make two applications of spray in order to provide residual amounts during this period. The first is put on before the leaves are developed in the spring and the residual DDT is presumed to remain effective until the time of the second application in July.

Preliminary investigations by the Bureau of Entomology and Plant Quarantine indicated that this procedure was an effective means of safeguarding valuable shade elms. However, frequent reports of bird mortality were received and it appeared advisable to review the program in terms of its effect on songbirds and other wildlife. This report summarizes results of the first summer's work on the problem. While the information was obtained from investigations in Princeton, New Jersey, the results would appear to be applicable to the Dutch elm disease problem as it exists elsewhere in the nation and particularly in several midwestern states.

[1] Formerly biologist with the Branch of Wildlife Research, U. S. Fish and Wildlife Service, and now Instructor in Biology, New York State Teacher's College, Albany, New York.

Acknowledgments

The work reported here was carried on as part of a cooperative program between the Bureau of Entomology and Plant Quarantine, U. S. Department of Agriculture, the Fish and Wildlife Service, U. S. Department of the Interior, the Borough of Princeton, New Jersey, and Princeton University. Wildlife investigations were under the immediate supervision of J. P. Linduska. Contributions to the early planning of the study were made by Paul F. Springer. Thanks are due also to B. H. Wilford and H. J. Dodd of the Bureau of Entomology and Plant Quarantine; L. T. Sanders, who had charge of the actual spraying operation; I. R. Riker, engineer of the Borough of Princeton; and Thomas Clarke and Karl Zapf of Princeton University. I am indebted also to R. H. Carter and R. C. Roark of the Bureau of Entomology and Plant Quarantine for their analysis of the DDT content of birds.

HISTORY OF AREA

The spraying program in Princeton, New Jersey, was begun in August, 1947, when an application of a 1 percent DDT emulsion was applied to village elms. In 1948 the program continued with a spring application from March to May of a 2 percent DDT-water emulsion. In this application the average dosage per tree was 18.87 gallons, or approximately 3 pounds of DDT. On June 24 the summer spraying was started, using a 1 percent DDT-water emulsion and an average dose of 19.61 gallons per tree. This application of approximately 1.5 pounds of DDT per tree was completed on August 10.

Complaints of bird mortality were first received following the prefoliar spray of 1948. About a dozen birds were reported killed, but in no case was DDT definitely proven to have been responsible. Some land-owners, feeling that birds on their property were being seriously reduced, withdrew from the spraying program. After the 1948 summer spraying a few reports of bird mortality were received but again no definite proof was secured that DDT was the cause. In 1949 a 2 percent DDT spray was again applied during the period of March 22 to May 18. Arrangements by Bureau of Entomology and Plant Quarantine personnel to receive dead birds resulted in the recovery of 15 individuals.

PROCEDURE

Field investigations were aimed primarily at determining the effects of bark beetle control on adult songbirds and on nestling young which were being reared during and immediately following spray application. Three principal approaches were used.

1. With the help of many Princeton residents and the Grounds Department of Princeton University, intensive observations during and following spraying were made in most of the treated areas to determine the extent of immediate mortality. Individuals collected were saved to be analyzed later for DDT content.

2. To determine success in rearing young, a search was made for nests which would contain eggs or nestlings at the time of spraying. Due to the lateness of the summer spray application many birds had already finished nesting, thus reducing the number of nests available for study. However, a number of second nestings occurred and enough nests were found to furnish some data on the question of nestling mortality.

3. In order to measure population decline and also to compare population densities, a study area was set up inside the sprayed zone and a check area of similar habitat type was selected about a mile outside the sprayed area. Since nothing definite is known about previous bird population in the study area, it is not possible to state what effect earlier sprayings may have had. However, to insure a reasonably accurate measurement of the effects of the summer application in 1949, censuses were made of the study area and the check area during the month preceding and the month following spraying. Ten censuses before spraying and ten after gave a fair idea of the trend in bird populations and also gave some indication of the relative abundance of birds in a sprayed

and unsprayed area of somewhat similar type.

It should be pointed out that various circumstances in this control work added further complications to the usually difficult job of selecting comparable control and study areas. To a considerable extent the effectiveness of such an operation depends upon community-wide participation. Practically all elms within the limits of the town were scheduled for treatment and the location of a directly comparable non-treated area for control purposes proved to be a practical impossibility. A 20-acre area selected to serve as a check was located in the rural outskirts of the town. Elm trees were prominent in this situation but other species of trees also were present and the understory of wild shrubs and herbaceous growth contrasted with the well-tended ornamentals in the grounds of city lots and estates. While the area was of limited value for check purposes, it did provide an acceptable means for detecting any conspicuous population movements and changes in the behavior of birds that might not have been understood from a study only in the treated area.

The 20-acre study area was of irregular shape and located in the residential section of Princeton. Elm trees were distributed along most of the roadways bordering and traversing the plot. Practically all residences and estates included additional elms in the landscape scheme. Spraying was confined to elm trees only. However, the frequency of this tree in and around the plot was such that unlimited opportunity existed for birds to experience frequent and prolonged contact with DDT-treated vegetation.

DIRECT MORTALITY

Following spraying 26 birds were found dead or dying and were preserved for analysis. Dead mammals found in sprayed areas included one red bat (*Lasiurus borealis*) and one gray squirrel (*Sciurus carolinensis*). In several cases birds seen before death, and which later died, showed symptoms characteristic of DDT poisoning. These included tremors and loss of ability to fly followed by inability to walk. One grackle, first seen at 3:00 p.m. on July 12, was at that time able to run freely but could not fly. By 5:30 p.m. it was barely able to walk and it died at 6:05 p.m. The bat was found on its back in a state of severe tremors and was unable to fly. It lived for an hour before it was killed. At that time it was not noticeably weaker than when picked up.

It is not possible to state with certainty that all animals found dead were victims of DDT poisoning. However, 15 of the 26 birds found dead in the sprayed area were subjected to chemical assay to determine the presence of DDT. Tests were conducted using the viscera and muscular tissue triturated together and DDT was recovered in every case. Obviously, even if all those collected were killed by DDT, the effect on the total population in any one area would be negligible. Of the 26 birds found, 11 were from the census area and included 6 nestling and 4 immature birds and one adult grackle. This last was not actually one of the resident population. Due to the activities of gardeners, as well as continual searches by investigators in the area, it is believed that most of the dead birds on the census plot were recovered so that 11 losses on approximately 20 acres represent a

reasonably accurate picture of the mortality involved. The census area, however, had a higher concentration of elms than most other parts of the sprayed area and thus received a much greater amount of spray. It is likely, therefore, that mortality here was higher than in the sprayed area as a whole.

Eight other birds found dead in the area of spraying were not in a condition to warrant preservation. Most of them were run over or were so decomposed as to indicate that they had been dead for some time. It is likely that most, if not all, of them represented normal mortality not associated with DDT poisoning.

POPULATION LEVELS

The only available information on the bird population of the study area prior to the beginning of the spraying program several years before was obtained from residents interested in birds. All such individuals agreed that the current bird population was lower than it had been before the spraying program began but their estimates of the decline varied. It is possible that a reduction in numbers of insects caused a decrease in the population of insectivorous birds independent of any direct mortality.

To evaluate the immediate effects of DDT on the bird population, detailed censuses of singing males on territories were made in check and sprayed areas both before and after spraying. At the time of the control work, however, nesting was largely completed and an effort was made to supplement census work on nesting populations by means of counts of the total numbers of birds using the study plots. In the latter procedure transients as well as residents were recorded in 10 trips before spraying and 10 after spraying through the check and sprayed areas. The indices to numbers from these counts proved to be of little utility. Records were without consistency due to variable numbers of migrants which appeared in plots and further complications to the procedure resulted when flocks of grackles concentrated on roads and parkways to feed on dead and dying insects.

The best basis for a consideration of possible effects of spraying on birds would appear to involve those data obtained on the resident portion of the population. In the measurement of this stable element of the population, each bird seen or heard was noted on a map of the area and a new map was used for each census. This information, when transferred to maps for each species, showed individual territories as well as the number of pairs of each species present.

These data summarized in Table 1 show changes in total breeding population on study and check areas after spraying. An identical decrease of 19.6 percent is indicated for both units. The decrease on the check area was due largely to a reduction in numbers of warblers, cowbirds and orioles, whereas species which decreased on the study area and those found dead after spraying were almost entirely ground-feeding birds. In view of the known mortality which occurred with the spraying, it is difficult to reconcile the identical population decline in sprayed and unsprayed plots suggested by the pre- and post-spray censuses. In considering only those species which were present in

TABLE 1.—BREEDING POPULATIONS OF STUDY AND CHECK AREAS BEFORE AND AFTER SPRAYING. *—SPECIES COMMON TO BOTH AREAS

Species	Study Area No. of Pairs		Check Area No. of Pairs	
	Pre-Spray	Post-Spray	Pre-Spray	Post-Spray
Mourning Dove	2	3	0	0
Downy Woodpecker	0	0	2	2
*Crested Flycatcher	3	0	2	2
Phoebe	0	0	3	1
Least Flycatcher	0	0	2	0
Wood Pewee	0	0	4	5
Blue Jay	2	1	0	0
Carolina Chickadee	0	0	1	0
White-breasted Nuthatch	0	0	2	2
*House Wren	8	5	2	3
*Catbird	8	6	14	15
*Brown Thrasher	1	1	1	1
*Robin	10	10	6	6
*Wood Thrush	6	8	2	2
Starling	2	2	0	0
Yellow-throated Vireo	0	0	3	2
*Red-eyed Vireo	2	2	3	2
Yellow Warbler	0	0	3	0
Chestnut-sided Warbler	0	0	1	0
Yellow-throat	0	0	6	6
Redstart	0	0	2	1
English Sparrow	5	3	0	0
Orchard Oriole	0	0	1	0
Baltimore Oriole	0	0	4	0
Cowbird	0	0	8	3
Scarlet Tanager	0	1	0	0
*Cardinal	3	2	3	4
Rose-breasted Grosbeak	0	0	1	0
*Chipping Sparrow	1	1	1	1
*Song Sparrow	3	0	15	16
Totals	56	45	92	74
Percentage change from pre-spray counts	Decrease 19.6%		Decrease 19.6%	

both the treated and check areas, however, a more realistic trend of events is indicated. Among species common to both plots (starred in Table 1) a 6 percent increase occurred on the check area following spraying, while a 22 percent decrease occurred on the study area. Those which decreased on the study area were song sparrow, catbird, house wren, cardinal, and crested flycatcher. None of these showed any decrease on the check area.

There were few canopy feeders on the study area and orioles, warblers, chickadees, nuthatches, pewees, and vireos found on the check area were notably absent on the treated plot. Since these species are not resident on the sprayed unit, it is possible that previous spraying operations may have eliminated or discouraged these species from using the area. Further studies having the benefit of areas previously unsprayed might provide an answer to this question.

Throughout the post-spraying period the numbers of robins and wood thrushes on the study area remained high and none of these birds were found

dead. This is of interest since they outnumbered all other species except grackles, and they constantly fed in the road and along the roadside under the sprayed elms.

It should be noted that the check area had a much higher population at the outset than did the study area. Part of this difference was no doubt due to its being of a rural nature and less densely inhabited. The shrubbery was unpruned and luxuriant. If the original population of the study area could have been determined for the years before spraying was initiated, the figures on comparative populations of sprayed and unsprayed areas would be of considerable value. Since this was not possible, the figures serve to indicate only the immediate effect of spraying. Further censusing of the two areas in succeeding years should indicate whether there is a progressive decline of population in the sprayed area.

MODIFIED BEHAVIOR AND MORTALITY NOTES

A sudden decline in numbers of birds on the study area occurred on July 12, a week after spraying was started, and persisted for a week, when a gradual rise began. By the end of the census period the population was at about the pre-spray level. The reason for this sudden decline is not clear. It did not appear that the birds were killed since singing males reappeared in many of the territories. Apparently the birds had either left the area and were temporarily feeding elsewhere or else they were so silent and inactive as to remain unobserved. The former seems the more likely answer.

It is possible, also, that the birds later recorded on the area were not the same individuals as those found there early in the census period. Observations did not indicate that this was the case. In almost every instance the birds were heard in the same territories as formerly. In some cases birds which had not been recorded on the census of a particular day were observed during daily work on the area. Banding of birds to make them individually recognizable would make it possible to secure more specific data on this point.

Because spraying operations were performed over an extended period of time, it was difficult to determine the time interval between spraying and the recovery of dead birds. In the case of 21 birds, however, the lapse of time was determined with fair certainty. Beginning with the first day after spraying and on each of seven subsequent days the following numbers were recovered: 1, 0, 3, 7, 4, 2, 2, and 2. Eleven birds (52%) were found on the 4th and 5th days, while 15 (71%) were found by the end of the 5th day. This time interval to the period of maximum mortality agrees with that found by George and Stickel (1949)[2] in the course of similar investigations in Texas.

NESTLING SURVIVAL

In order to study nestling survival, nests were located on the check and study areas just prior to the spraying. Of those found on the sprayed area only three contained young when spraying began on July 5. These were 3

[2] George, John L., and William H. Stickel. 1949. Wildlife Effects of DDT Dust Used for Tick Control of a Texas Prairie. Amer. Midl. Nat. 42: 228–237.

catbird nests with a total of 8 young. Five young from 2 nests died on July 9 and 10 and the remaining 3 young from the third nest fledged on July 9. At least one was still alive and apparently normal on July 16. The two adults of this family also were seen at this time. In all, 6 nests containing 18 young were under observation in the sprayed area in the period during or immediately following spraying. Only 8 (44%) of this total were brought to a successful fledging. To obtain data on normal nestling mortality, 9 nests on the check area were watched until the eventual fate of the nestlings was known. Of the 21 young under observation 15 (71%) fledged. While the numbers of individuals involved in both cases are small, an appreciably higher mortality is indicated for young in the treated area.

SUMMARY

During the summer of 1949 studies were conducted at Princeton, New Jersey, to determine the effects on wildlife of DDT used in the control of Dutch elm disease.

Direct mortality was determined by intensive search for dead birds after spraying. Twenty-six songbirds, one bat, and one gray squirrel were found. Of 11 dead birds from a study area of approximately 20 acres only one was an adult.

Songbird populations were determined by censuses before and after spraying on a study area and on a similar unsprayed check area. The number of all breeding birds showed a 19.6 percent decrease on both the study and check areas during the week immediately following spraying. About two weeks after spraying the population in the treated area began to increase and within another week was back to pre-spray level. When only those species common to both sprayed and unsprayed areas are considered a population decline of 22 percent was measured in the sprayed area while the check area showed a 6 percent increase in numbers over the same period.

Nestling mortality was studied in both areas. Among 18 young found on the study area during and following spraying 8 (44%) survived. Death of two broods of catbirds occurred four days after spraying. Of 21 young found on the check area 15 (71%) survived.

RECOMMENDATIONS

With the knowledge secured from this study it is possible to make a few general recommendations which should help to minimize the hazards to wildlife of this particular control program. In the spring period it is advisable that spraying be completed prior to April 15. All birds found dead during the spring of 1949 were recovered after April 20 although spraying had been going on since March 24. This indicates that, had spraying been completed by April 15, there would have been little bird mortality. Effects on squirrels during the early spring period are less easily controlled since at that time of year their food supply is limited and they partake largely of elm seeds and buds. However, no appreciable loss of squirrels has yet been reported and it is possible that such a problem may not appear in serious proportions.

The summer spray should be timed to coincide with the slacking off of nesting. This occurs at Princeton near the first

of July and after that date the number of nestling birds is comparatively small. It would be desirable to limit the operation to only the spring application if reasonable control of the disease could thereby be obtained.

At all times care should be taken to cover bird baths and other possible sources of drinking water for birds, to prevent contamination by DDT.

During spraying operations it is important to remove all contaminated puddles as soon as possible. Those in the street could be dispersed by a street-sweeping machine while those on private property more logically would be the responsibility of the individual owners.

As is recommended for any such use of toxic compounds, dosages should be the absolute minimum consistent with acceptable control.

Accepted for publication March 28, 1950.

OBSERVATIONS ON DEER KILL UNDER DIFFERENT SYSTEMS OF HUNTING

Kaj Westerskov[1]

Danish Game Research Station, Kalø pr. Rønde, Denmark

A comparison between deer population density and the annual harvest of white-tailed deer (*Odocileus virginianus*) in Michigan, under the public hunting system, and in Denmark, under the Old World system, yields data of interest for the biologist, the sportsman, and the game administrator.

During a stay in Michigan deer country in September 1948, it occurred to the author that the harvest of deer in Michigan was much lower proportionally than in the author's home country, Denmark. Discussions of the matter with Michigan deer biologists, I. H. Bartlett and G. A. Amman were very stimulating. The author also wishes to acknowledge his great indebtedness to Dr. Daniel L. Leedy of the U. S. Fish & Wildlife Service for valuable criticism of the manuscript.

In 1945 the deer herd was estimated at nearly 800,000 head (Ruhl, 1946: 147) in Michigan, and the deer kill that year was the largest on record, namely 97,721 deer, or 12 per cent of the total population. In 1945 the combined deer herd in Denmark was estimated at 2,044 red deer (*Cervus elaphus*), 866 sika deer (*Pseudaxis sika*), 3,881 fallow deer (*Dama dama*), and about 80,000 roe deer (*Capreolus capreolus*) (Alex-Hansen, 1945:229). A few moose (*Alces europæus*) are found, but are protected. This gives a total of 86,791 deer of which 19,126 deer were reported killed to the game department (Stat. Dept., 1947:138), equal to 22 per cent of the herd, or almost double the harvest percentage in Michigan.

For comparison it may be added that

[1] Formerly of the Ohio Wildlife Research Unit, the Ohio State University (1947–1949), now leader of game bird research, Danish Game Research Station, Kalø pr. Rønde, Denmark.

the Swedish moose herd in 1945 was estimated at 46,995 (Hamilton, 1945: 170) of which 11,014 (Anm., 1945:513), or 23 per cent were reported killed.

The reasons for the proportionally higher kill of deer under the Old World system is undoubtedly mainly due to four causes: first, the administration of Danish forest lands involving a close connection between forestry and game management and resulting in a closer check on the deer herd; second, much longer seasons; third, no bag limits; and fourth, the fact that Danish sportsmen only hunt on their own land, leased land, on land where they are guests or where they are employees.

Regarding the first reason it may be added that the foresters and game keepers in the various forest districts, estates, and larger farms keep track of the deer herd from year to year, follow the reproduction and increase in the herd and calculate where possible the number of animals to be taken. Also the severity of the foregoing winter is taken into consideration.

The intensity of the deer management and hunting varies according to type of land, ownership, size of area, and mainly the interest of the sportsman who possesses the hunting right.

On the estates where the game keepers are in charge of the game management, deer are shot according to a plan. Late-born fawns are usually killed, as they are not figured to be strong enough to stand the winter anyway. Weak bucks with poorly formed antlers and old bucks with long pointed antlers (the so called "murderers") are killed if possible. Old barren does are killed if known. Further sick-looking, small and meager deer are shot. Thus a qualitative as well as a quantitative selection is carried out; the herd is harvested and all surplus removed without reducing the capacity of the herd, and the qualitative selection is carried out as briefly discussed above. Only occasionally (as when honouring an esteemed guest) is the ideal buck with beautiful, large, well shaped antlers removed. It will be understood that the type and shape of the antlers is of primary importance, although the meat, of course, is also valued greatly. Evidence of this great interest in the development of the antlers is the fact that Danish sportsmen always mount the antlers with the upper part of the skull and keep all the antlers on the walls in their office, library or hall. On each skull is written date and area where killed, and insignia of the sportsman, and on the back of the plate one of the jaws is inserted to make an age check possible.

Also on smaller hunting grounds deer are harvested after similar rules, and the sportsmen spend much time watching deer also before the season opens to find out which ones to kill, where they are to be found, and acquaint themselves with the habits of the deer.

It is highly probable that a greater proportion of the total deer herd can be killed annually in Michigan. The requirements for this greater kill, namely a better knowledge of the deer herd, of its numbers, age, health conditions, development, sex ratio, etc., on the part of the sportsman is not present in most cases, since many deer hunters in Michigan just come to the deer country in the open season to hunt deer. The fundamental differences between Michigan and Denmark in this respect: the many forests open to public hunting in

Michigan and the hunting right invested in landownership in Denmark may be so large that a compromise between the methods cannot be reached.

The second factor mentioned was the much longer open seasons in Denmark, thus enabling sportsmen to a more thorough harvest of the herd. As a matter of interest to American sportsmen and biologists the open seasons for Danish deer are shown below. It may be added that the open seasons are fixed in the game law (the latest Danish game law of 1931) and do not change from year to year.

1. Red deer. Bucks may be hunted from September 1–February 28; does and fawns from October 1–February 28.

2. Sika deer. Bucks may be hunted in the summer from May 15–July 14, and again from October 1–December 31. Does and fawns from October 1–December 31.

3. Fallow deer. The same seasons as mentioned for red deer.

4. Roe deer. The same open seasons as mentioned for sika deer.

Third, there is no bag limit on any game species in Denmark. In other words in the open season, within the hours of hunting (from sunrise till senset), on land where he has the right to hunt, the Dane has the legal right to bag any number of any legal game species. The regulation of the harvest is thus more one of true sportsman-spirited origin than a legal restriction.

The fourth factor involved is the fact that the country consists of a large number of large and small hunting units (state and private forest districts, estates, farm woodlots, medium and small sized farms, hunting club grounds) on which only the owners, hunting right leasers, guests or personnel do hunt. In this way there is a close contact between the people hunting the areas, and the harvest is controlled and followed. All larger-sized hunting estates and forest districts keep shooting records in which all animals bagged are listed. Usually extensive management methods are carried out on their areas, involving winter feeding, protection of seedlings and young trees, fencing of the whole district, feeding of saltlicks, cutting of especially young trees in the wintertime thus making browse available, etcetera.

Because of the large difference in size between Michigan and Denmark a direct comparison between the deer herds of the two areas is not possible, but some comparative data might be of interest to see how deer fare under different systems of hunting.

In Michigan in 1945 there were 800,000 deer on 58,216 square miles, or 14 deer per square mile; in Denmark there were 87,000 deer on 16,575 square miles, or 5 deer per square mile. But whereas the forest land only occupies 9 per cent of the total land area in Denmark, 52 per cent of Michigan is in forest land.

In Denmark there is a herd of 87,000 deer on 1344 square miles of forest land, or 65 deer per square mile of forest land. In Table 1 is shown the number of deer found, number of deer killed, percentage of kill, and amount of dressed meat produced.

Because of the size difference in the white-tail and the various Danish deer (red deer are larger, roe deer much

TABLE 1.—THE DANISH DEER HERD IN 1945 (ALEX-HANSEN, 1925:229); KILL (STAT. DEPT., 1947:138), PERCENTAGE OF KILL AND AMOUNT OF DRESSED MEAT

Species	Total herd	Reported kill	Percentage kill	Dressed meat
Red deer	2,044	346	17%	64,875 lbs.
Sika deer	866	176	20%	15,840 lbs.
Fallow deer	3,881	1,478	31%	203,225 lbs.
Roe deer	80,000	17,126	21%	659,351 lbs.
Total and averages	86,791	19,126	22%	943,291 lbs.

smaller), a "pound of meat produced per unit area" comparison is suggested. In Denmark 943,291 lbs. of dressed meat are harvested on 1344 square miles of forest land, or 702 lbs. per square mile, equal to a little more than 5 white-tailed deer killed per square mile of forest land.

Michigan had by 1945 a herd of about 800,000 on 29,686 square miles of forest land, equal to 27 deer per square mile (compared with 65 deer per square mile of Danish forest land). The 1945 legal deer kill in Michigan was 97,721 head equal to 12,703,730 lbs. of dressed meat, based on Hamilton's (1947:349) figures of an average of 130 lbs. for a dressed deer. This amount of deer is equal to a production of harvested deer of 428 lbs. per square mile (compared with 702 lbs. per square mile of Danish forest land), or about 3 white-tailed deer per square mile of Michigan forest land.

In order that the harvest of deer in Michigan should equal the one in Denmark an additional 8,133,964 lbs. should be harvested, equal to 62,569 deer. In that way the percentage of deer bagged in Michigan would be about 20, the total amount of dressed meat bagged 20,837,694 lbs.

After this mere supposition the following might be of interest. At the training school held by the Michigan Game Division at Higgins Lake in January 1949, which meeting the author had the privilege to attend, the deer managers worked out a plan as to how many more deer they figured should and could be killed in their respective districts, taking into consideration number of deer in the yards, present kill, carrying capacity of the yards, and winter mortality. After much deliberation the final result was tabulated, and ran close to 60,000.

If the technician's assumptions are right (and they were unfamiliar with the Danish conditions here described), and if the Danish 20–25 per cent kill is optimum, then it cannot be a mere coincidence that the author's calculated increased kill, amounting to 62,569 deer, and that calculated by the Michigan deer biologists to keep the herd in equilibrium with the deer yards, amounting to 60,000 deer, ran so close.

In Michigan as well as in Denmark poaching of deer takes place, to how great an extent is not known, neither here nor there. That factor, therefore, may be outruled as a maybe more or less stable decimating factor in both areas, and beyond the reach of checking. Winter mortality occurs in both places, but is much heavier in Michigan, partly because of the lesser kill so that too many deer are left for the food

available, partly because a selective removal of weaklings in the Michigan deer herd does not take place.

These data can be interpreted as indicating that under the present deer hunting system in Michigan, the crop of deer is not harvested to its full extent, three deer being killed per square mile of forest land, compared with the equivalent of five white-tailed deer being killed per square mile of Danish forest land. An increase of about 60 per cent in the kill, as believed possible by the Michigan deer men and calculated by the author in analogy with Danish conditions, would bring the annual deer kill in the two remote areas in equilibrium, about 700 lbs. of dressed meat being bagged per square mile of forest land.

The Danish deer kill has ranged 20–25 per cent without decreasing the herd. The available data seems to show that the same can be done in Michigan.

The problem is, then, to regulate the greater deer kill, and this in itself is probably a greater problem than managing the deer. In Denmark all deer can be killed, bucks, does, and fawns, but the various sex and age groups are, as far as possible, harvested according to the plan described above. How an open season can be set, so that the ratio between the sex and age groups of Michigan deer will be regulated by the kill is beyond the scope of this paper. Under a public hunting system as practiced in Michigan it will be difficult; under the Danish hunting system it is possible.

SUMMARY

In Denmark a herd of 86,791 deer of four species was found in 1945; 19,126 deer, or 22 per cent of the herd were bagged. In Michigan a herd of about 800,000 white-tailed deer were found in 1945; 97,721 deer, equal to 12 per cent, of the herd were bagged.

The reasons for the proportionally higher kill of deer under the Old World hunting system may be classified thus: first, the close cooperation between forestry and game management resulting in a closer check on the herds; second, the much longer seasons; third, no bag limits; fourth, the hunting right invested in landownership, so that comparatively few hunt, and hunt according to a plan, removing the surplus and weaklings, and regulate the sex and age ratio.

It is considered highly possible that a greater proportion of the Michigan deer herd can be removed annually. In Michigan in 1945 there were 27 deer per square mile of forest land; in Denmark there were 65 deer per square mile of forest land. Because of differences in body size of the deer species in question the comparison is given on a "pound of meat produced per unit area" basis. Michigan produced 428 lbs. of dressed meat, and Denmark 702 lbs. per square mile of forest land.

If the Michigan deer harvest was increased 60 per cent it would equal the Danish removal of deer. About 60 per cent increase is also what Michigan deer men have calculated should be killed to bring the deer herds into equilibrium with the deer yards.

The greatest problem involved seems to be the regulation of the hunting of deer in Michigan, securing an appropriate ratio between the sex and age classes, and removal of undesirable elements of the deer herd, such as late

born fawns, weaklings, bucks with poor antlers and barren does.

LITERATURE CITED

ALEX-HANSEN, BØRGE. 1945. Det klovbærende Vildts Udbredelse i Danmark. Dansk Jagttidende, **62**: 227–229.

ANONYMOUS. 1945. Årets älgjakt. Svensk Jakt, **83**: 513.

HAMILTON, HARRY. 1945. Älginventeringen 1945. Svensk Jakt, **83**: 168–173.

HAMILTON, W. J. JR. 1947. Dressed Weights of Some Game Mammals. Journ. Wildl. Mgt., **11**: 349–350.

RUHL, H. D. 1946. Game Division, in Michigan State Department of Conservation; 13th Biennial Report: 125–161.

STATISTISKE DEPARTMENT. 1947. Vildtudbyttet i 1945–1946. Dansk Jagttidende, **64**: 137–138.

Accepted for publication March 23, 1950.

WISCONSIN PHEASANT REPRODUCTION STUDIES BASED ON OVULATED FOLLICLE TECHNIQUE[1]

Irven O. Buss, Roland K. Meyer and Cyril Kabat

State College of Washington, University of Wisconsin and Wisconsin Conservation Department

Pheasant investigations show that populations on different areas vary in many aspects of reproduction. An understanding of these variations will result in more intelligent pheasant management.

Ovulation studies of artificially propagated ring-necked pheasants (*Phasianus colchicus* Linnaeus) have been conducted and reported (Meyer, Kabat and Buss, 1947; Kabat, Buss and Meyer, 1948). It was concluded that the remains of ovulated follicles are valid criteria for determining: (1) Ovulation incidence, (2) ovulation rates and (3) number of eggs laid from the onset of laying up to six months postovulation. Although the above studies dealt primarily with game-farm birds, it was concluded that this technique could be applied to the study of productivity in wild pheasant populations.

In this report data obtained from application of the ovulated-follicle technique are presented. Also, pheasant-reproduction information is given including the average date of first egg, laying rate, total eggs laid and the number of clutches abandoned before penned birds began incubation. This reproduction information and the ovulated-follicle data are compared with the analyses of ovaries collected from two wild populations occupying different types of habitat.

Unfortunately, it was impossible to study the same aspects of reproduction in these two wild populations and the penned birds simultaneously and with parallel objectives. Nevertheless, the studies yielded new information on old problems previously investigated through

[1] This work was supported in part by a grant-in-aid from the Wisconsin Alumni Research Foundation and by the Pittman-Robertson pheasant research project 9-R. The authors acknowledge the cooperation of the Wisconsin Conservation Department which furnished pheasants and pens for experiments, and Drs. Helmut K. Buechner and Donald S. Farner for critical reading of the manuscript.

nest studies and general observations in the field.

MATERIALS AND METHODS

Pen experiments. Three groups of artificially propagated pheasants were used in experiments conducted in 1943 and 1944. The data obtained were presented in a report by Kabat, Meyer and Buss (1948). Some of these data have been arranged in tabular form for further study of laying behavior and in graphic form for comparison with similar data from wild pheasants. In addition, data on reproduction for these same three groups of pheasants were obtained incidentally to the studies of ovulation. Pen experiments were continued after 1944 to obtain supplementary data on the dates when first eggs were laid. As in 1943 and 1944, hens were penned with cocks in both small (12′ X 12′) and large (100′ X 25′) pens where their egg-laying behavior was observed.

Wild pheasant population study areas. The two areas, University Arboretum and University Bay, selected for this study are located adjacent to the city of Madison, Wisconsin. They were selected because they differ in type of habitat, and also because considerable history is available on their pheasant populations.

The University of Wisconsin Arboretum is a refuge with about 600 acres of habitable winter range and is located at the south edge of Madison. The area includes 500 acres of marsh, 60 acres of oak-hickory timber and 40 acres of old fields. It has been ungrazed, unmowed, unshot and unburned since 1932; it offers very dense winter cover. Artificial feeding has been practiced in varying degrees; a trap mortality equivalent to light shooting has been sustained, and the area serves as winter range for the pheasants of a larger adjoining summer range (Leopold, Sperry, Feeney and Catenhusen, 1943). The population density increased progressively from 133 pheasants in 1937–38 to 402 pheasants in 1942–43 (Buss, 1946). Since then this population has declined drastically, and for the years 1945–49, it has averaged less than 200 birds. Simultaneously, the cover has increased in both density and distribution.

The University Bay area of 550 acres is a refuge adjoining the west edge of Madison, including 230 acres of upland woods, orchard, roadsides, idle real estate and two small marshes. As at the Arboretum, birds on the Bay area are artificially fed to varying degrees. Unlike the Arboretum, approximately half of the Bay area is intensively cultivated, largely for crop experiments, and it probably serves as winter range for the pheasants of a restricted adjoining summer range.

Collection and examination of ovaries. Seventy wild female pheasants were collected during April, May and June in 1943, and 12 during June, 1944, from the University of Wisconsin Arboretum, to obtain data on the laying performance of hens from an area with a dense population and nearly impenetrable shrub-tree cover for comparison with similar data from game-farm pheasants.

Fifteen wild hens were collected during the summer of 1946 from the University Bay area to obtain data on laying that could be compared to the data obtained from the University Arboretum collections.

Soon after the hens were shot, their ovaries were removed and fixed in a solution of 30 parts of formaldehyde, 10 parts of 95 per cent alcohol, 10 parts of glacial acetic acid, and 50 parts of water. A few were examined immediately. Counts of ovulated follicles were made at various intervals following fixation. Since practically all of the hens in 1943 were laying at the time of collection it was relatively easy to count ovulated follicles and to estimate the number of eggs laid up to the time of shooting. In studying ovulation of confined hens, it was found that a few pigmented structures were produced by non-ovulated involuting follicles (Kabat, Buss and Meyer, 1948). Since the number of these structures was very small, and because they can usually be recognized if involution has not progressed beyond 15 days, it was assumed that each ovulated follicle represented an egg laid.

The date of first egg for the 12 hens in 1944 was more difficult to estimate. Six of the hens were still laying at the time of collection, at least three were with broods and three had finished laying immediately prior to collecting. The effect of these variables collectively on estimating the date of laying first eggs is assumed to cause a deviation similar to that which arises in taking a sample from game-farm birds. The estimated date of first egg for hens with broods was calculated by subtracting from the collection date: (1) The age of the broods, (2) 23 days for incubation and (3) the length of the laying period (number of eggs laid multiplied by 1.3. See laying rates).

RESULTS AND DISCUSSION

Before any attempt was made to interpret data on reproduction of the two wild-pheasant populations, similar information from pen-reared pheasants was analyzed and used as a guide for treating wild-population observations. Therefore, the presentation of the data on reproduction accumulated for game-farm hens precedes that for wild birds.

Reproduction in Game-Farm Hens

Date of laying first eggs. Studies were conducted during 1943–49 to determine the average date of first eggs laid and the factors affecting this date in game-farm hens (Table 1). Although the hens

TABLE 1.—FIRST EGG LAYING DATES FOR WISCONSIN PHEASANTS

Period within which first eggs were laid	Number of Hens Starting							Wild birds 1943*	
	Game Farm Birds (1943–49)					Total		Total	
	1943	1944	1947	1948	1949	No.	Percent	No.	Percent
April 1–7			2			2	.8	6	8.8
April 8–14	6	6	11	10	6	39	14.3	4	5.9
April 15–21	30	15	29	35	16	125	46.7	19	28.0
April 22–28	7	8	16	22	16	69	25.8	25	36.8
April 29–May 5	3	4	6	8	12	33	12.4	14	20.5
Total	46	33	64	75	50	268	100.0	68	100.0
Average Date						April 21		April 22	
Medium Date						April 20		April 27	

* Estimated first egg dates for all hens from the University Arboretum and Bay Areas, 1943, 1944, and 1946 collections.

observed in each year's study were different birds, the annual frequency distributions of first laying dates are very similar. Annual observations show that a very high percentage of all hens observed started to lay by mid-April. These data indicate that the onset of laying was more closely associated with seasons than weather which differed during the winter and spring prelaying period. It is generally recognized that increased light from longer days stimulates reproduction in birds. Bissonnette and Csech (1936) have shown that pheasants can be induced to lay ahead of schedule by increased lighting. Numerous other reports show that by increasing the daily amount of light a high rate of egg-laying can be promoted during winter.

The difference in first-laying dates for individual game-farm hens observed yearly varied up to 30 days. The first-laying dates for wild hens probably vary even more since age ratios are apt to differ with time and place. It has been shown for the domestic fowl that laying begins later in each successive year, that laying is in large measure controlled by the individual bird's heredity and the date when a hen lays her first egg varies according to the season when she is hatched (Romanoff and Romanoff, 1949). The possibilities for wide variation in first-laying dates are apparent and may be very important in the interpretation of data on reproduction in wild pheasants.

Laying rates. In 1944 two groups of pheasants were penned in enclosures with one hen to each cock. In the first group, consisting of 13 pairs confined separately in large pens, 440 eggs were laid in 584 days for an average of 1.3 days for each egg laid. The average number of eggs laid by these hens was 36 with a range of 15 to 44 eggs. The second group, consisting of 54 pairs confined separately in small pens, laid 1,350 eggs in 1,694 days, again an average of 1.3 days for each egg laid. In 1943 and in the 1947–49 period 285 birds were placed in enclosures using five or more hens per cock; the hens showed laying rates identical with those observed in 1944. Shick (1947) reports 111 eggs laid in 155 days or 1.4 days for each egg laid.

Consequently, the laying periods for game-farm birds were determined by multiplying the ovulated-follicle counts by 1.3, and first-egg dates were deduced by subtracting this laying period from the date of last ovulation. Since similar data for wild pheasants are not available, this same correction is used to determine laying periods for these birds.

Laying and nesting behavior. Table 2 shows the number of eggs laid at random and in nests by 11 hens confined in large pens at the State Experimental Game and Fur Farm during 1944. It shows that they laid from 4 to 27 eggs at random (average 12.5) after which they laid up to 24 additional eggs in nests (11 eggs per hen) which were never incubated. Following abandonment of these nests, the hens laid from 6 to 13 eggs in nests (average 9.9) which were incubated. In all, the total laid by these hens varied from 15 to 42 and averaged 34. The range in number of eggs laid prior to incubation (columns two and three) is comparable to the difference in first-egg dates (Table 1) and indicates wide variation in individual behavior of these hens. This variation suggests that the initiation of laying in these

game-farm pheasants is not directly related to the phenomena causing incubation especially since the hens started laying at practically the same time.

Table 2 also shows the number and size of clutches laid by the 11 hens. These pheasants laid from one to four clutches, and 12 of the 13 hens laid clutches in nests that were abandoned for reasons that appeared to be completely unrelated to environment. A total of 36 clutches were laid by the game-farm hens, but only 13, or 36 per cent, were incubated. If this desertion behavior is typical for wild hens, it can readily be seen that considerable error has probably been made interpreting nest losses based on field studies.

Many nesting studies have been made in the field that show typically high losses to nests. Some of these are shown below:

References	Locality	Nests studied	Percentages unsuccessful
Bach (1942, p. 110)	No. Dak.	258	73
Bach (1942a, letter)	No. Dak.	314	68
Bach (1948, p. 208)	No. Dak.	88	86
Basket (1947, p. 17)	nc Iowa	391	75
Buss (1946, p. 47)	s Wis.	350	61
Buss & Swanson (1950)	se Wash.	68	72
Carlson (1942, p. 25)	Minn.	271	71
Eklund (1942, p. 225)	Oregon	145	55
English (1933, p. 29)	Mich.	193	65
Fetherston (1949, p. 105)	Pelee Is.	251	70
Hamerstrom (1936, p. 191)	nw Iowa	445	77
Kimball (1946, p. 173)	So. Dak.	51	75
Kimball (1947, letter)	So. Dak.	461	88
Randall (1940, p. 312)	se Penn.	310	80
Shick (1944, p. 147)	Mich.	87	70
Stokes (1949, p. 2)	Pelee Is.	1194	53
Strode & Leedy (1940, p. 1)	Ohio	563	42
Twining (1947, p. 185)	Calif.	171	78

The percentage of success for the 5,611 nests shown is 34. It is noteworthy that this percentage of successful nests is similar to the percentage of nests (36) incubated successfully by the game-farm birds reported in this study. Either the game-farm hens deserted an abnormal number of clutches, or the high mortalities reported for wild hens includes more actual desertion than was realized.

Most of the published reports show analyses of unsuccessful nests, and invariably desertion is included as one of the causes. It is highly probable that the percentage of failures classed as desertion is too low. The losses attributed to predators unquestionably included nests abandoned before depredations occurred. It would be impossible in many cases to tell whether a nest was deserted and then destroyed, or destroyed and then deserted. Another source of error in determining the cause of nest failures occurs in the cases where incubation had begun and hens were killed while off their nests, thus exposing these nests and leaving them more vulnerable to predation. Destruction of such nests would lead observers to record them as losses due to predation. Blaming predators for such nest

TABLE 2.—LAYING PERFORMANCE OF 11 PHEASANTS CONFINED IN LARGE PENS, 1944

Bird Number	Eggs Laid* at Random	Eggs Laid in Nests Not Incubated	Eggs Laid in Nests Incubated	Total No. of Eggs Laid
62	27	2, 6	7	42
63	23	2, 5	11	41
64	7	3, 4	11	25
65	14	20	10	44
66	11	5, 6	13	35
67	9	2, 7	11	29
68	12	6, 12	6	36
72	4	3, 11, 10	12	40
73	19	5, 3	10	37
74	6	5, 5, 4	7	27
75	5	0	10	15
Averages	12.5	5.8	9.9	34

* Laid singly and mostly prior to nest formation.

mortalities and those actually caused by desertion invites mismanagement.

Analysis of Reproductive Behavior in Populations of Wild Pheasants

Although the game-farm hens confined in pens simulating natural conditions laid an average total of 34 eggs, they incubated clutches that averaged 10 eggs. This clutch size is similar to the average number of eggs reported in field studies. Is the date of first egg, laying rate, and total laid by wild pheasants and game-farm birds also similar? What is the relation of this behavior to differences in productivity of pheasant populations between areas?

Involution rates of ovulated follicles. Examination of ovaries from wild pheasants containing ovulated follicles in various stages of involution indicated a rate of resorption that was almost identical with rates measured in game-farm birds. The weights in milligrams of the ovulated follicles of three pheasants collected at the University Arboretum are shown below.

No. 15: 188, 93, 60, 16, 9, 3, 3, 1, –1, –1
No. 17: 180, 74, 28, –1, –1
No. 19: 175, 87, 56, 25, 14, 8, 6, 5, 3

The weights of these involuting ovulated follicles is very similar to the weights shown for game-farm hens (Meyer, Kabat and Buss, 1947). These data furnish additional evidence that the ovulated-follicle technique developed for game-farm birds can be used to analyze data on reproduction obtained from wild populations.

Date of laying first eggs. The dates of first eggs for the wild hens collected from the University Arboretum and Bay areas as calculated from counts of ovulated follicles are shown in Tables 3, 4 and 5. It is apparent that the hens collected from the University Arboretum, 1943, and University Bay, 1946, started to lay at a similar time, whereas the laying dates for the Arboretum in 1944 appeared to be considerably later. It is possible that some of the 1944 hens started to lay early, incubated clutches early, then lost the first clutch and renested. The ovaries of such hens might contain relatively few ovulated follicles and thus indicate a late date of first egg laid unless the bird's past history was known. This explanation might account for the relatively late first-egg dates for the 1944 Arboretum hens.

TABLE 3.—OVULATION HISTORY OF 44 WILD HEN PHEASANTS COLLECTED AT THE UNIVERSITY ARBORETUM, 1943

Bird Number	Date Bird was Killed	No. of Ovulated Follicles	Laying* Period (Days)	Date of First Ovulated Follicle
2	4/27	3	4	4/23
3	4/27	5	7	4/20
4	4/27	2	3	4/24
5	4/27	6	8	4/19
6	4/28	2	3	4/25
7	4/28	1	1	4/27
8	4/28	6	8	4/20
9	4/28	2	3	4/25
10	4/28	7	9	4/19
12	4/29	9	12	4/17
13	4/29	7	9	4/20
14	4/29	6	8	4/21
15	4/30	10	13	4/17
16	4/30	2	3	4/27
17	4/30	5	7	4/23
19	4/30	9	12	4/18
21	5/4	20	26	4/8
22	5/4	19	25	4/9
23	5/4	6	8	4/26
26	5/5	7	9	4/26
27	5/5	6	8	4/27
29	5/6	9	12	4/24
30	5/6	5	7	4/29
31	5/6	7	9	4/27
32	5/6	14	18	4/18
33	5/6	9	12	4/24
35	5/8	10	13	4/25
36	5/10	22	29	4/11
38	5/10	27	35	4/5
39	5/10	13	17	4/23
47	5/13	2	3	5/10
49	5/13	13	17	4/26
76	5/14	17	22	4/22
77	5/14	15	20	4/24
79	5/21	37	48	4/3
80	5/21	21	27	4/24
82	5/27	21	27	4/30
83	5/29	37	48	4/11
85	6/13	39	51	4/23
86	6/13	42	55	4/19
90	6/13	36	47	4/27
91	6/13	16	21	5/23
92	6/16	36	47	4/30
93	6/26	50	65	4/22

* Obtained by multiplying the number of ovulated follicles by 1.3.

To compare first-egg dates of wild and game-farm hens, data from all wild hens collected are shown in Table 1. The frequency distributions of starting dates are similar with the possible exception that wild birds started laying somewhat later than the game-farm birds. Whether this difference be real or biased by errors of sampling, its effect on the respective populations would be insignificant.

Dates of first eggs laid by 84 game-farm hens in 1943 and 1944 have been plotted in Figure 1. A comparison of

laying dates for the 1943 hens with those of 1944 showed a difference of three days in first-egg dates and a difference of six days between peaks of most frequent laying of first eggs. Since these differences are not considered significant, the dates for both years are plotted as a single curve. The figure also shows frequency curves of first eggs laid by the hens in the University Arboretum in 1944, first eggs laid by the hens in the University Bay

TABLE 4.—OVULATION HISTORY OF 12 WILD-HEN PHEASANTS COLLECTED AT THE UNIVERSITY ARBORETUM, 1944

Bird Number*	Date Bird was Killed	No. of Ovulated Follicles	Laying** Period (Days)	Date of First Ovulated Follicle
1	6/10	22	29	5/12
2	6/13	42	55	4/19
3	6/13	19	25	5/19
4	6/13	56	73	4/1
5	6/27	29	38	5/20
7	6/27	29	38	5/20
8	6/27	25	33	5/25
9	6/27	27	35[1]	4/16
10	6/27	19	25	6/2
11	6/29	43	56	5/4
12	6/29	23	30[1]	4/23
13	6/29	43	56[2]	3/14
Average		31		

* Numbers 1, 2, 3, 4, and 7 were laying at the time of collection.

** Obtained by multiplying the number of ovulated follicles by 1.3.

[1] With two-week chicks, hence their age plus 23 days for incubation have been added to the laying period to deduce date of first ovulated follicle.

[2] With four-week chicks, hence their age plus 23 days for incubation have been added to the laying period to deduce date of first ovulated follicle.

TABLE 5.—OVULATION HISTORY OF 12 WILD-HEN PHEASANTS COLLECTED AT UNIVERSITY BAY FARMS, 1946

Bird Number	Date Bird was Killed	No. of Ovulated Follicles	Laying* Period (Days)	Date of** Last Ovulated Follicle	Date of First Ovulated Follicle
4	6/29	21	27	5/3	4/6
10	7/13	15	20	5/10	4/20
14	7/26	21	27	4/27	3/31
15	7/26	15	20	6/5	4/16
18	8/6	12	16	5/3	4/17
20	8/6	12	16	5/5	4/19
27	8/13	15	20	5/12	4/22
28	8/16	23	30	5/17	4/17
29	8/21	16	21	5/20	4/29
30	8/27	20	26	5/26	4/30
32	8/29	13	17	5/28	5/11
35	9/16	22	29	5/19	4/20
Average		16			

* Obtained by multiplying the number of ovulated follicles by 1.3.

** Deduced by subtracting age of the hen's brood and the incubation period from the collection date.

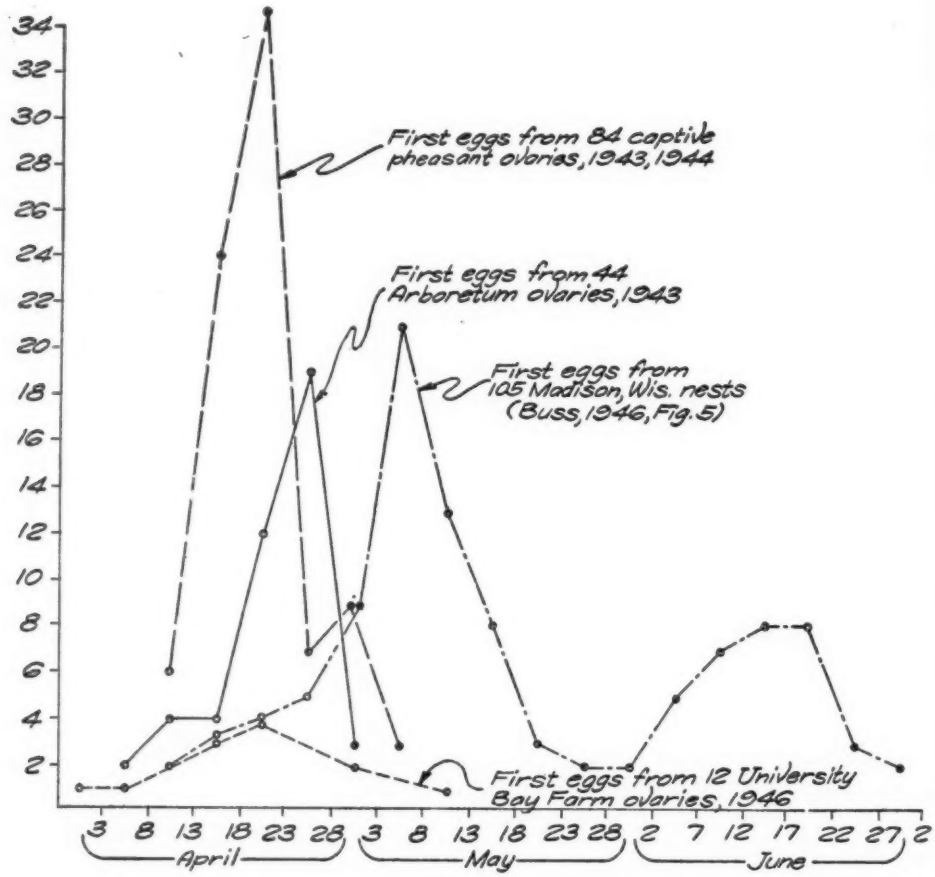

FIG. 1. Distribution of 245 First Laying Dates for Wisconsin Pheasants.

area in 1946 and first eggs laid in 105 nests at the Madison Fish Hatchery area, about two miles south of the Arboretum (Buss, 1946).

In each group of pheasants, one or more started to lay between April 3 and 13, indicating that wild and artificially propagated pheasants responded alike to the phenomena that initiated laying. Laying records kept at the State Experimental Game and Fur Farm for general egg production and for the experimental birds (Table 1) show that the average year-to-year difference between first eggs laid by game-farm pheasants varies but a few days.

The peak of laying first eggs occurred between April 18 and 28 for the University Arboretum, the University Bay area and the game-farm hens. The first egg dates for the wild birds are based on examination of ovaries. However, the peak of first eggs *laid in nests* for pheasants at the Madison Fish Hatchery occurred between May 3 and 8, which is an average difference of 13

days from the date of first eggs laid for the above groups. A second peak occurred in the Madison Fish Hatchery curve between June 12 and 22 averaging 55 days later than the first eggs laid by the other three groups. The actual date of the first eggs laid is unknown, but is probably similar to that for the above groups.

On the basis of these four groups of data, it appears that both wild and artificially propagated pheasants start laying at approximately the same time in spring and that considerable difference occurs between individual hens from the same areas.

Egg laying rate, total eggs and nesting. Information on egg-laying rates of wild birds is limited to nest observations and the progressive decrease in size of ovulated follicles of any one ovary. This evidence indicates a strikingly similar laying rate between the wild and game-farm birds.

Evidence for the total number of eggs per pheasant indicates that at least some hens lay more eggs than are found in the average clutch. Almost all people working with pheasants have reported findings of "dump" nests ranging up to 39 eggs which were laid by two or more hens. However, the average number of eggs laid by hens, including those laid at random and in unsuccessful nests, remained completely unknown.

Ovulated-follicle data from ovaries of 44 hens collected at the University Arboretum in 1943 are summarized in Table 3 which shows that these wild pheasants laid up to 50 eggs. The hens killed during April were laying, but relatively few ovipositions had been made. Those killed in May exhibited considerable variation in number of eggs laid, but they showed an increase proportional to the time of collection. Three hens (Nos. 21, 38 and 79) which had completed laying during May averaged 28 eggs. The size of the largest ovulated follicles in these three hens indicated that they had ceased laying less than one week prior to the time of collection (Meyer, Kabat and Buss, 1947). Four of the six hens collected in June were still laying at the time of collection, yet these six pheasants had laid an average of 37 eggs. The high follicle counts indicate that these hens must have laid almost continually from the date of first egg to the date of collection to have attained their egg-laying record. It is apparent that these wild hens had a laying pattern very similar to the game-farm hens, and they laid far more eggs than the average clutch reported for nests of wild pheasants. Does the 1943 Arboretum pheasant population behavior represent an average picture? The data collected from the Arboretum in 1944, University Bay in 1946 and in scattered places 1946–49 will help answer this question.

The data from 12 wild hens collected from the University Arboretum in 1944 are summarized in Table 4. These collections were made in June in order to obtain hens in advanced stages of reproduction. The average of 31 ovulated follicles is very similar to the 37 eggs laid by the hens in June, 1943. These averages show that some wild hens lay in excess of two times the number found in nests. However, additional data obtained at the University Bay in 1946 and at scattered sites during the period 1946–49 show different averages.

Hens were collected at the University Bay in 1946 (Table 5) after they

had ceased ovulating. Most of these hens began laying during April like the game-farm and Arboretum birds, but they laid an average of only 16 eggs which is about half the number laid by both populations at the Arboretum. Is this contrast due to difference between years or difference between ranges?

Although spring and summer collections were not made at the Arboretum in 1946, there are some indications that many eggs were again laid by this population. A few data regarding laying behavior of the Arboretum hens were obtained incidentally during February, 1947 when dogs killed four hens on the area. Examination of their ovaries revealed counts of 37, 21, 26 and 32 ovulated follicles. These counts indicate relatively large numbers of eggs laid since many ovaries have follicles involuted beyond the stage of identification at this late date.

Supplementary information on total eggs laid by pheasants supports the conclusion that the number laid varies between areas and years and that many more eggs are laid than are found in nests. The peak of first-egg dates is shown in Figure 1 for hens from the Fish Hatchery as determined from nest studies. If the Fish Hatchery birds laid at the same rate as the game farm pheasants (1.3 days per egg) during the 13– and 55–day periods preceding the two peaks, from 10 to 42 eggs were laid at random or in deserted nests by these wild pheasants. This would bring the total number of eggs laid far above the 15 eggs found in the average nest at the Fish Hatchery in 1941.

Examination of ovaries from numerous hens on highways in various areas during June, 1946–49 showed that all of these hens had laid and that about half of them were still laying during this month. The range of estimated number of eggs laid was 11–56. A hen killed by a mower in Milwaukee County, 9 September 1947 during a third alfalfa cutting, was incubating a clutch of 14 eggs. Her ovary contained 56 ovulated follicles. The actual date that this hen started laying would be almost impossible to calculate, but it is obvious that several clutches were laid. It is apparent that the number of eggs laid by different populations varied considerably and extremes occurred in two areas only one mile apart.

Analysis of reproduction rates. To determine whether wild hens laying many eggs abandon nests at a rate similar to game-farm hens in pens simulating wild habitat, it is necessary to estimate the total eggs laid from counts of ovulated follicles of wild birds. The average number of eggs laid by wild hens is shown in Tables 2, 3 and 4, which show that many more eggs were laid than found in nests and that some of the hens had laid continuously without cessation. Either these hens laid all of their eggs at random, failed in first and second, and in some third nests, or they abandoned some nests. The latter seems to be the more probable explanation since a relatively large number of nests were found containing eggs with no evidence of incubation. If predators destroyed nests destined for incubation, more eggs would also be laid by such hens than found in nests. The reason for differences in egg-laying behavior between areas is not clear. The relatively large number of eggs laid by hens in the Arboretum in contrast to hens in the University Bay

area indicates that range and population factors may be involved.

This raises the question whether pheasant populations containing hens that lay many, or those that lay few eggs are more productive. The productivity of the hens in the University Arboretum and University Bay area for 1946–49 is shown in Table 6. The smaller brood size, lower percentage of young birds and higher percentage of broodless hens indicate lower productivity for the hens in the Arboretum than those in the Bay area. The earlier studies on these areas showed contrasting numbers of eggs laid by these populations, hence it is probable that this phenomenon was associated with reproduction for years 1943, 1944 and 1946. Table 6 also shows that on the average the birds in the Bay area ceased laying earlier, as indicated by their hatching dates. It appears that range differences are currently responsible for the disparity in productivity between the Bay and Arboretum. The nearly continuous shrub-tree cover on the Arboretum has gradually become exceptionally dense, forming impenetrable labyrinths to humans in many places. An increasing proportion of the vegetation is near-climax status, and the area lacks interspersion of tilled fields with weedy edges which occur on the Bay area. Pheasants are typically associated with early successional stages, hence it is probable that the advanced stage of succession on the Arboretum is responsible for the difference in productivity that occurred between the two areas.

Relationship of date of first egg to hatching dates. The frequency distribution of hatching dates, as determined by observations of broods during the summer of 1949 at the University Bay and Arboretum, are shown in Table 7 for comparison with dates of first eggs for both the game-farm and wild birds. Considering time interval and percentage of birds that hatched each week, the distribution of hatching dates is very similar to that shown for dates of first egg. However, there is no evidence of a correlation between the peak of hatching dates and the date of first egg for game-farm birds. This is demonstrated by data on the date of first egg and the first day of incubation for 20 hens confined in pens simulating wild-nesting habitat (Kabat, Buss and Meyer, Tables 1 and 2), which showed no evidence of correlation. The number of hens beginning incubation late is

TABLE 6.—SUMMER PHEASANT PRODUCTIVITY OBSERVATIONS*

	Average Hatching Date		Average Brood Size		Broodless Hens (Per Cent)		Young/adults (Per Cent)	
Year	Bay	Arboretum	Bay	Arboretum	Bay	Arboretum	Bay	Arboretum
1946	5/29	6/15	5.4					
1947	6/9	6/15	5.4	3.9	25	38	67	53
1948	6/10	6/1	6.0	5.0	2	22	72	71
1949	6/15	6/22	7.5	4.8	3	16	81	71

* Kozlik (1949).

TABLE 7.—FREQUENCY DISTRIBUTION OF 112 HATCHING DATES AS DETERMINED BY SUMMER BROOD OBSERVATIONS—1949

Hatching Dates	Sixty-two University Bay Broods[1]		Fifty University Arboretum Broods[2]	
	Number	Per Cent	Number	Per Cent
5/20—5/26	1	1.6		
5/27—6/2	8	12.9		
6/3—6/9	16	25.9	2	4.0
6/10—6/16	11	17.7	15	30.0
6/17—6/23	12	19.3	15	30.0
6/24—6/30	9	14.5	9	18.0
7/1—7/7	5	8.1	7	14.0
7/8—7/14			1	2.0
7/15—7/21			1	2.0

[1] Average hatching date for University Bay 6/15.
[2] Average hatching date for University Arboretum 6/22.

divided equally among the early and late layers. These data on hatching dates and dates of first eggs yield additional evidence to support the observation made previously that initiation of laying in pheasants is not directly related to the phenomena causing incubation.

Perhaps the variation between dates on which hens begin laying and incubating on different areas and during different years is characteristic for pheasants. Studies of the bobwhite quail in Wisconsin (Thompson and Kabat, 1949; Thompson, 1949) suggest that this reproduction behavior may be a characteristic inherent to the species. These studies show that the peak of hatching and the frequency distribution of hatching dates for bobwhites, which extend from June to October, are even more variable between different years than for the pheasant. It is remotely possible that the current land uses are implicated in the variation that occurs in the hatching dates between areas and years for both quail and pheasants.

SUMMARY AND CONCLUSIONS

The date of first egg laid by wild female pheasants can be estimated by examination of ovulated follicles if they are collected while laying, incubating, or with broods. Year after year a very high percentage of all game-farm and wild hens observed began laying by mid-April in Wisconsin.

Variations in winter and spring weather had little or no effect on laying dates of first eggs, hens began laying at practically the same time and apparently the stimulus which initiated egg-laying was not directly related to the phenomena which cause incubation.

Laying eggs at random and deserting one or two nests probably represents typical behavior for a wild hen; this behavior has resulted in gross misinterpretation of the typically high nest mortalities reported in many studies.

Wild hens have a laying pattern similar to game-farm hens, and both lay far more eggs than the average clutch reported for wild pheasant nests. The number of eggs laid by pheasants varies between areas and years.

Comparison of populations on two dissimilar areas in southern Wisconsin showed that high egg production correlated with relatively low reproduction on one area, with directly opposite conditions occurring on the other area. This difference in productivity is believed to be the result of difference in range conditions on the two areas.

LITERATURE CITED

*BACH, ROY N. 1942a. Wildlife survey and investigation. Pittman-Robertson Quarterly, U. S. Dept. of the Interior, Fish and Wildlife Service, **2**(1): 110–111.

———. 1942b. Letter, dated September 22, 1949.

———. 1948. Wildlife survey and investigation. Pittman-Robertson Quarterly, U. S. Dept. of the Interior, Fish and Wildlife Service, **8**(2): 208–209.

BASKET, THOMAS S. 1947. Nesting and production of the ring-necked pheasant in north-central Iowa. Ecol. Mon., **17**: 1–30.

BISSONNETTE, THOMAS H. and ALBERT G. CSECH. 1936. Eggs from pheasants and quail induced by night-light. Science, **83**(2156): 392.

BUSS, IRVEN O. 1946. Wisconsin pheasant populations. Wis. Conservation Dept. Bul. 326, 184 pp.

BUSS, IRVEN O. and CARL V. SWANSON. 1950. Some effects of weather on pheasant reproduction in southeastern Washington. Trans. N. A. Wildlife Conf., **15**: 364–377.

*CARLSON, C. EDWARD. 1942. Upland game division. Pittman-Robertson Quarterly, U. S. Dept of the Interior, Fish and Wildlife Service, **2**(1): 25–27.

EKLUND, CARL R. 1942. Ecological and mortality factors affecting the nesting of the Chinese pheasant in the Willamette valley, Oregon. Jour. Wildlife Mgt., **6**(3): 225–230.

ENGLISH, P. F. 1933. Causes of pheasant mortality in Michigan. Unpublished Ph. D. Thesis, Univ. of Mich. Library, Ann Arbor.

FETHERSTON, KATHLEEN E. 1949. A study of the ring-necked pheasant on Pelee Island, Ontario. Unpublished Ph.D. Thesis, Univ. of Cornell Library, Ithaca.

HAMERSTROM, FREDERICK N., JR. 1936. A study of the nesting habits of the ring-necked pheasant in northwest Iowa. Iowa State Col. Jour. of Science, **10**(2): 173–203.

KABAT, CYRIL, IRVEN O. BUSS and ROLAND K. MEYER. 1948. The use of ovulated follicles in determining eggs laid by the ring-necked pheasant. Jour. Wildlife Mgt., **12**(4): 399–416.

*KIMBALL, JAMES W. 1946. South Dakota pheasant study. Pittman-Robertson Quarterly, U. S. Dept. of the Interior, Fish and Wildlife Service, **6**(4): 172–173.

———. 1947. Letter, dated August 22, 1949.

KOZLIK, FRANK M. 1949. Pheasant-quail management research. Wis. Wildlife Research, Quarterly Progress Report, Madison, **8**(3): 23–49.

LEOPOLD, ALDO, THEODORE M. SPERRY, WILLIAM S. FEENEY and JOHN A. CATENHUSEN. 1943. Population turnover on a Wisconsin pheasant refuge. Jour. Wildlife Mgt., **7**(4): 383–394.

MEYER, ROLAND K., CYRIL KABAT and IRVEN O. BUSS. 1947. Early involutionary changes in the post-ovulatory follicles of the ring-necked pheasant. Jour. Wildlife Mgt., **11**(1): 43–49.

RANDALL, PIERCE E. 1940. The life equation of the ringneck pheasant in Pennsylvania. Trans. N. A. Wildlife Conf., **5**: 300–320.

ROMANOFF, ALEXIS L. and ANASTASIA J. ROMANOFF. 1949. The avian egg. John Wiley & Sons, Inc., New York, 918 pp.

*SHICK, CHARLES. 1944. Habitat requirements of the ring-necked pheasant. Pittman-Robertson Quarterly, U. S. Dept. of the Interior, Fish and Wildlife Service, **4**(4): 147–152.

———. 1947. Sex ratio—egg fertility relationships in the ring-necked pheasant. Jour. Wildlife Mgt., **11**(4): 302–306.

STOKES, ALLEN W. 1950. Pelee Island pheasant project. News Letter No. 4, Mimeographed report, 4 pp.

STRODE, DON H. and DANIEL L. LEEDY. 1940. The 1939 pheasant nesting study in Wood County, Ohio. Ohio Wildlife Research Station, Columbus, Release No. 135, August 5, 20 pp.

THOMPSON, DONALD R. 1949. Bobs away. Wis. Conservation Bul., 14(5): 17–20.

THOMPSON, DONALD R. and CYRIL KABAT. 1949. Hatching dates of quail in Wisconsin. Jour. Wildlife Mgt., 13(2): 231–233.

*TWINING, HOWARD. 1947. The life history and management of the ring-necked pheasant in California. Pittman-Robertson Quarterly, U. S. Dept. of the Interior, Fish and Wildlife Service, 7(4): 185.

* Permission has been secured from authors to cite data first reported in Pittman-Robertson Quarterlies.

Accepted for publication April 18, 1950.

PRODUCTION AND UTILIZATION OF WATERFOWL FOOD PLANTS ON THE EAST TEXAS GULF COAST

J. R. Singleton

Texas Game, Fish and Oyster Commission, Austin, Texas

During the year June 1946 through May 1947, thirteen species of well known waterfowl food plants were observed and studied to determine production in terms of pounds of food produced per acre. An attempt was made to determine the environmental factors which tend to limit or increase production. Methods used in this study were patterned after Low and Bellrose (1944). Certain modifications, discussed below under Field Methods, were necessary since this study covered a period of one year, while Low and Bellrose (1944) collected numerous samples from a wide variety of habitats during the summer months when the seeds were mature.

Ludlow Griscom (1947) has urged that additional research and improvement in technique are needed in estimating the plant and animal food crop on the waterfowl winter range—in other words, its carrying capacity. With this thought in mind, the writer has attempted to analyze the plant food crop on selected plots on the Texas Gulf Coast, one of the most important wintering areas for waterfowl on the North American continent.

The plan was to select meter quadrats of each species; to make weekly inspections of each quadrat; to collect, air-dry, and weigh material; and to record all pertinent information that might be of value in determining productivity. Data were recorded on water depth and turbidity, type and extent of pest damage, burning, competition, and seasonal growth. For an eight week period salinity of surface water was determined. This was discontinued for two reasons: first, many of the plots were so located as to be subjected to tide water or its influence. The salinity of surface water from these plots varied so greatly from week to week that individual determinations were of little value. Secondly, Penfound and Hathaway (1938) state that salinity of soil water (not surface water) and water level appear to be the important habitat factors.

This investigation was conducted in

Orange, Jefferson, and Chambers Counties in extreme southeastern Texas. Fresh water marshes, slightly brackish marshes, and brackish marshes are present in this area. Although the study was limited to this portion of the coast, it is felt that the results are applicable to other sections.

ACKNOWLEDGEMENTS

Grateful acknowledgement is made of the assistance of many interested parties who made this study possible, particularly Walter P. Taylor, formerly leader of the Texas Cooperative Wildlife Unit, John J. Sperry, Department of Biology, Agricultural and Mechanical College of Texas, College Station, Texas, and Daniel W. Lay, Texas Game, Fish and Oyster Commission, Silsbee, Texas, who gave their time and experience to supervise and guide field techniques; W. B. Davis, head of the Department of Wildlife Management, Agricultural and Mechanical College of Texas, College Station, Texas for assistance in compilation and preparation of data; the Texas Game, Fish and Oyster Commission, Austin, Texas for permission to use waterfowl stomach data, and A. C. Martin, Arnold Nelson, and Francis Uhler, United States Fish and Wildlife Service, Patuxent Refuge, Laurel, Maryland, for suggestions and assistance in analyzing waterfowl stomachs.

FIELD METHODS

An extensive survey of the area was made to determine the abundance and availability of various waterfowl foods. The findings of Low and Bellrose (1944), and Martin and Uhler (1939) were consulted in the final selection of fourteen plant species to be studied (Table 1). Fifty-five meter plots were staked.

Plots were set in situations typical for the species to be studied; for some species the plots were in pure stands, for others in mixed stands. Both maxi-

TABLE 1.—PLANTS SELECTED FOR STUDY, NUMBER OF PLOTS, PORTION OF PLANTS COLLECTED, AND AVERAGE YIELD

Plant species	Number of plots	Portion collected	Average pounds per acre (dry)	Average pounds per acre (wet)
		Corms	9,580	11,990
Scirpus robustus	7	Seed	299	
Scirpus Olneyi	5	Seed	24	
Scirpus californicus	5	Seed	44	
Ruppia maritima	5	Stems & leaves	1,757	
Echinochloa Walteri	5	Seed	813	
Cladium jamaicense	3	Seed	582	
Pontederia cordata	3	Seed	346	
		Stems & leaves	6,394	
Leersia oryzoides	5	Seed	137	
Potamogeton foliosus	3	Stems & leaves	600	7,367
Polygonum hydropiperoides	3	Seed	581	
Rhynchospora corniculata	3	Seed	910	
Eleocharis quadrangulata	3	Seed	8	
Echinodorus cordifolius	2	Stems & leaves	535	5,940
Salicornia herbacea	3	Stems & leaves	680	

NOTE: In those cases where plants produce achenes or grains, they are called seed in this report, even though they are technically known as fruits.

mum and minimum production areas were selected.

Except where noted otherwise food material weight is given in grams of air-dried material.

Martin and Uhler (letter, September 26, 1946) suggested that wet weights of food materials be determined as well as the air-dry weights since plants in general are largely water, the aquatic plants particularly so, and that wet weights would be more comparable to quantities consumed by wildlife. The majority of data had been collected previous to this date; however, all material collected after receiving the suggestion was weighed when wet and again when air-dry.

All material was gathered by hand. Seeds were collected as they matured. In some cases, all seed on a plot matured at the same time making only one collection necessary. In cases where gradual ripening occurred, seeds were collected several times during the fall. An example of the latter is pickerelweed (*Pontederia cordata*), seed being gathered as many as five times from a single plot. A modified method for the collection of gradually ripening seed of smartweed (*Polygonum hydropiperoides*) was developed as follows: seed clusters on each plot were counted, then 100 of these clusters were collected at random from each plot. Seeds on each 100 clusters were counted and averaged to arrive at the average number of seed per cluster. When this figure was derived for each plot, the weight of clean air-dried seed per cluster per plot was found. From this determination, the weight of seed produced on each plot was estimated. As an example of this method, the calculations for plot 1 are shown below.

Plot 1 has 974 seed clusters. One hundred clusters had 1,504 seeds, making an average of 15.04 seeds per cluster. Weight of 15.04 air-dry seeds (or 1 cluster) was .0445 gram. Weight of 1 cluster times 974 clusters equals total estimated yield of 43.34 grams of smartweed seeds from plot 1.

Where yield of stem and leaves was to be determined, all of the plant above the soil surface was collected from each meter quadrat; where rootstock yield was to be determined, all of the plants below the soil surface to a depth of six inches was collected from each meter quadrat.

ENVIRONMENTAL FACTORS

Meteorological Data

All of the plots included in the present study were within a 50 mile radius of Beaumont, Texas. The area is one of essentially coastal climate. General climatic conditions over a period of three years are shown in Table 2, and individual variations are shown in Figure 1.

TABLE 2.—GENERAL METEOROLOGICAL CONDITIONS 1944, 1945, AND 1946*

Year	Temperature Mean Annual	Rain Fall Inches	Relative Humidity	Last Spring Killing Frost	First Fall Killing Frost	Growing Season In Days
1944	69.5	70.72	80.4	Feb. 12	Nov. 30	291
1945	69.6	65.39	81.4	Jan. 10	Nov. 24	318
1946	69.6	84.82	81.3	Feb. 15	Dec. 21	309
Long Term Average	68.5	56.12	72.7	Feb. 27	Nov. 26	272

* Texas Agricultural Experiment Station, Sub-station 4, Beaumont, Texas.

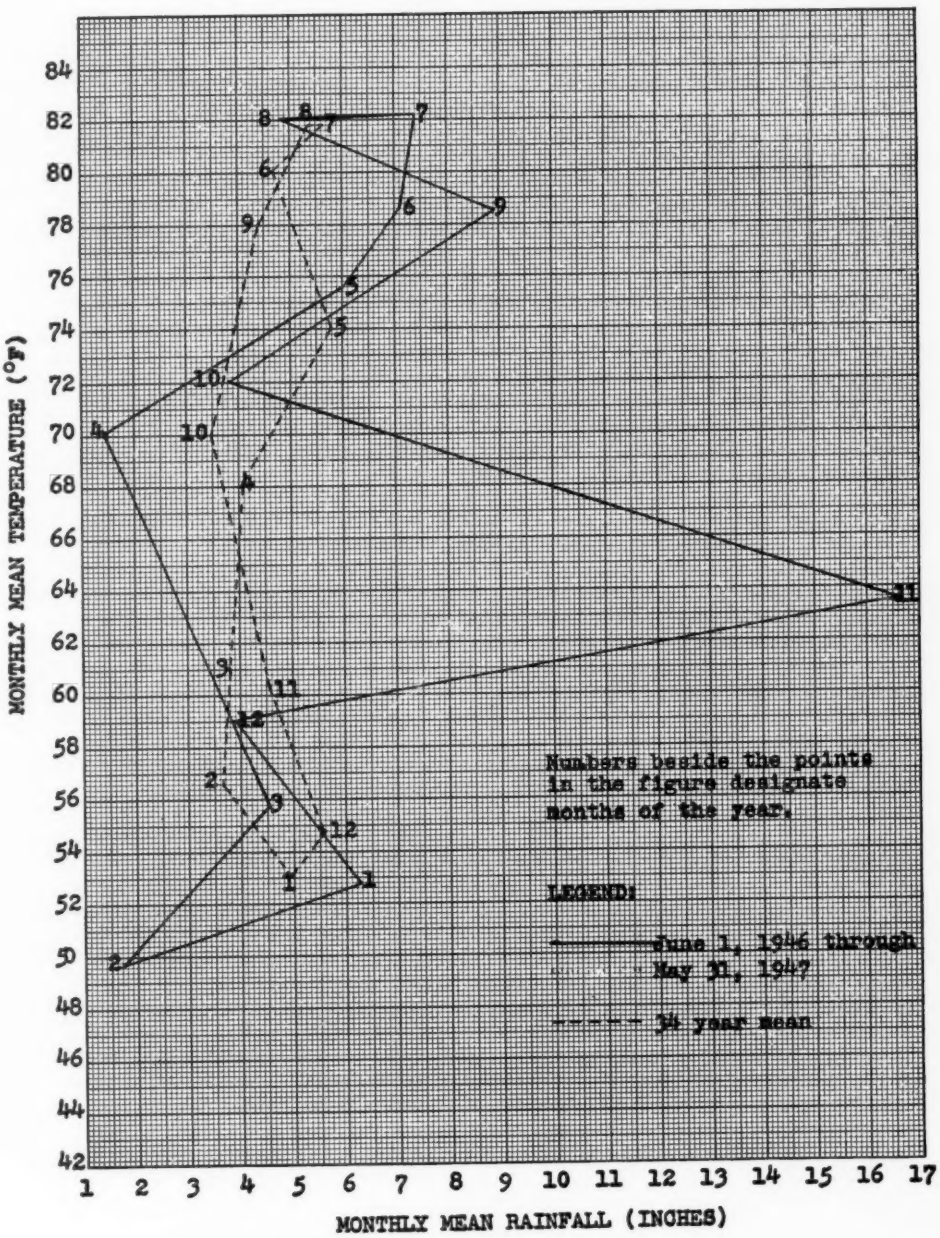

FIG. 1. Record of rainfall and temperature.

While the long-term average growing season was 272 days, the annual growing season in 1946 was 309 days, the average for the 3 years 1944 through 1946 was 306 days. This extended growing season undoubtedly accounts for some species being observed in bloom almost every day in the year.

The writer has not attempted to determine the importance of the physiographic factors because there was very little if any variance in these factors discussed below.

In only one case were the plots on more than one soil type, that being southern bulrush. Yield figures do indicate that soil factors limited the productivity of southern bulrush (Table 3).

Martin and Uhler (1939) state that irregular or extreme fluctuation in water level is the most important single factor in preventing the development of waterfowl feeding grounds. Low and Bellrose (1944) found that water depth and water turbidity were foremost among the environmental factors affecting the normal yield of seed and foliage.

Variables in water depth and water turbidity were insufficient to make a conclusive study of the effects of these two factors; however, the data collected may show a significant trend of the factors.

Pickerelweed plots showed the greatest variance in fluctuating water levels—the water depth varying from 5 inches to 36 inches.

Water depths on plots of widgeon grass varied only from 11 inches to 19 inches. These plots of widgeon grass were located in small ponds having a semistabilized water depth.

The increased yield of southern bulrush from plot 5 was caused by the more stabilized water level. The water depth on plot 5 varied from 9 inches to 18 inches while water depths on plots 1, 2, 3, and 4 of southern bulrush varied from 0 to 17 inches. As previously stated, Penfound and Hathaway (1938) found water level and salinity of soil water to be the most important habitat factors.

As was the case in water depth, the data on turbidity do not lend themselves to interpretation due to the absence of variance. Turbidity readings ran very similar on plots for a given species and, therefore, preclude any inferences as to the limiting role of turbidity, even though other investigators have found turbidity to be an important factor in growth.

LABORATORY ANALYSIS

Seed Yields by Weight. All plant material was collected, air-dried, and weighed in grams. Seeds were cleaned by rolling them in a rubber inner-tube. Chaff and plant debris were separated by use of an aspirator and graduated

TABLE 3.—SOIL TYPES AND AVERAGE YIELD OF SOUTHERN BULRUSH

Plot Number	1	2	3	4	5
Yield of seed in grams	4	1	3	3.6	12
Soil type	Marsh silty clay	Marsh silty clay	Madeland	Madeland	Lomalto clay
Average gram yield	2.5		3.2		13

sieves. Whenever possible, seed were weighed and analyzed in the same condition as when utilized by waterfowl. Gram weights rather than volumetric measurements were used.

A total of 79 samples of food material were collected from June 1, 1946 to June 1, 1947. Care was taken to collect all seeds from each plot as they matured. After being air-dried, chaff and plant debris were separated from the seed. Despite reasonable care, some chaff remained with the seed.

After yields from each plot were collected and treated, the average plot yield was determined to arrive at a more accurate figure from which acreage yields could be calculated. Pounds per acre were determined by multiplying the "grams per meter" by the coefficient 8.92. Number of square meters in an acre (4049) divided by the number of grams in one pound (453.6) equals 8.92.

yields and data on plant competition lend themselves to isolating the role of competition as a limiting factor on productivity. One of the best examples of this was in the yield of beak rush seed (*Rynchospora corniculata*) (Table 4). Dallis grass (*Paspalum dilatatum*) and smartweed (*Polygonum hydropiperoides*) were the competing plant species in the plots of beak rush and do indicate that competition is definitely a limiting factor upon productivity. As the degree of competition increased, the yield of the species of particular interest did decrease.

The heaviest yield of wild millet seed was from plots having the lowest percentage of competing plants, thus indicating the effects of competition upon productivity (Table 5). Salt grass (*Distichlis spicata*) was the competing plant.

Yield of leafy three-square seed does not lend itself to interpretation as well as the other species discussed. Burning in previous years may have been a contributing factor in seed production of leafy three-square, but since records of exact acres burned are unavailable, this possibility cannot be verified. Plots 4 and 5 of leafy three-square were burned on February 8, 1947. This burning seemed to have no effect on

TABLE 4.—YIELD, PERCENTAGE COMPOSITION, AND COMPETING PLANTS IN BEAK RUSH PLOTS

Plot Number	1	2	3
Yield of beak rush seed in grams	140.	54.5	112.2
Percentage stand of beak rush	96%	50%	80%
Other plants in beak rush plots	4% Dallis Grass	40% Dallis Grass 10% Smartweed	20% Smartweed
Average gram yield, 3 plots	102.23		

Yield as Affected by Plant Competition. Martin and Uhler (1939) point out that the effects of one plant upon another merits much more careful attention than has previously been given to it, with special emphasis upon the effects of an undesirable species upon a species which is valuable as a waterfowl food plant.

Only in a few instances did plot

TABLE 5.—YIELD, PERCENTAGE COMPOSITION, AND COMPETING PLANTS IN WILD MILLET PLOTS

Plot Number	1	2	3	5	5
Yield of wild millet seed in grams	51.	103.	96.	103.	103.
Percentage composition of wild millet	50%	75%	95%	95%	95%
Other plants in wild millet plots	50% salt grass	25% salt grass	5% salt grass	5% salt grass	5% salt grass
Average gram yield	77		100.67		

the plants since the first evidence of spring growth was observed on April 5, 1947 in all five plots of leafy three-square.

Plant Growth as Affected by Burning. Only three instances of burning occurred in the plots under study. Of these, the burning of leafy three-square, which is discussed above, apparently had no effect upon spring growth and subsequent plant blooming and seed production.

Three sawgrass plots were burned by the State Highway Department on September 19, 1946. By October 19, 1946 the burned plants had put out from six to eight inches of new growth. On January 4, 5, and 6, 1947 when the new growth was two to four feet in height, the ends of all the leaves were frozen for a distance of at least one foot from the tip.

On May 22, 1947, large stands of sawgrass in the vicinity which were not burned on September 19, 1946 were bearing immature doughy seed, while the burned plots gave no evidence of seed stalks, much less blooms or seed.

One distinct advantage in periodically burning sawgrass is that it makes the seed available to waterfowl. Under normal unburned conditions, sawgrass grows in such thick rank stands that it is virtually impossible for waterfowl to swim among the plants and pick up the seeds.

Three plots of smartweed were burned on February 8, 1947. All of plot 1 and approximately two-thirds of plot 2 were burned to the extent that none of the old dead stems were left. One-third of plot 2 and all of plot 3 were only lightly burned so that much of the old dead stems were left. On March 15, 1947, the first evidence of spring growth of smartweed was observed on plot 1 and two-thirds of plot 2, but no evidence of spring growth on the remainder of plot 2 or on plot 3. It was not until April 22, 1947, that any indications of spring growth was noted on the lightly burned portion of plot 2 and on plot 3. Apparently the seedlings suffered no ill effects from the burning.

Vegetative Yield by Weight. Stems and leaves of emergent species of plants greatly outweighed stems and leaves of submerged plant species. The air-dried yield of widgeon grass was much greater than the yield of pondweed. The average yield of emerged plant species was 3,537.7 pounds per acre, while the

average yield of submerged species was only 964.2 pounds per acre.

WATERFOWL STOMACH ANALYSIS

As previously explained, the species of plants studied are among those growing in southeastern Texas which, according to Martin and Uhler (1939) and others, are important duck foods in the region.

Two hundred and thirty-eight waterfowl stomachs were collected on the Jackson Ranch in Chambers County, Texas from November 22, 1946 to January 6, 1947. These birds were killed in a brackish marsh, less than 200 acres in size, characterized by three grasses, cord grass, and salt grass. Large beds of dwarf spike-rush occurred in shallow brackish ponds and seemed to be especially attractive to ducks and coots.

Two hundred and three waterfowl stomachs were collected during the periods of November 4 to 17 and December 16 to 29, 1947. These birds were collected from seven localities, all of them being in the marshes of southeastern Texas.

Of the 441 waterfowl, 363 were dabbling ducks of eight species, 16 were diving ducks of three species, and 31 were geese of four species. The contents of the stomachs collected were analyzed according to the method discussed by Martin, Gensch and Brown (1946).

Results of stomach analysis show that of the plants investigated spike-rush, wild millet, widgeon grass, smartweed, and pondweed are definitely of great importance as waterfowl food plants. Bulrush and sawgrass are of secondary importance. (Table 6.)

TABLE 6.—MAJOR FOOD PLANTS OF 410 DUCKS FROM SOUTHEASTERN TEXAS, 1946–48*

Food Item	*Times Used*	*Volume %*
Rice (*Oryza sativa*)	159	39.99
Dwarf spike-rush (*Eleocharis parvula*)	117	8.32
Wild millet (*Echinochloa Walteri*)	46	7.84
Widgeon grass (*Ruppia maritima*)	86	4.29
Smartweed (*Polygonum hydropiperoides*)	54	3.98
Square stem spike-rush (*Eleocharis quadrangulata*)	116	3.27
Pondweed (*Potamogeton foliosus*)	52	2.86
Duckmeat (*Lemna minor*)	51	2.08
Sawgrass (*Cladium jamaicense*)	73	1.89
Sedge (*Cyperus ferax*)	124	1.83
Primrose willow (*Jussiaea diffusa*)	38	1.55
Gulf coast spike-rush (*Eleocharis cellulosa*)	141	1.44
Pondweed (*Potamogeton americanus*)	28	1.27
Sedge	20	1.23
Cyperaceae tubers		
Jungle rice (*Echinochloa colonum*)	25	1.14

*The animal items in these ducks totalled 7.1%. Snails comprised 3.16%, crustacea 1.86%, insects 1.24%.

The greatest percent of the diet of ducks was rice, this being 39.99% by volume of all foods taken and occurring 159 times in 410 stomachs. Martin and Uhler (1939) found that rice made up only 3.60% of the diet of ducks examined by them occurring 110 times in 2,101 stomachs. This difference can be explained in part by the wider range of collection of Martin and Uhler, whose stomach collection records date back to 1901, and are widely extended geographically. Furthermore there has been a marked increase in rice acreage in Texas, namely from 291,000 acres harvested in 1940 to 400,000 acres harvested in 1947. Undoubtedly a proportional amount of this increase in rice acreage has been made on areas which were formerly utilized by wintering waterfowl. This large percentage of rice in the diet has evidently replaced a portion of the natural foods taken in previous years, as the data of Martin and Uhler (1939) do show a higher

percentage of natural foods as compared with the present study.

The results of the stomach analysis do point out the importance of determining proper management practices of the major food items, so as to increase their abundance in the waterfowl winter range. The six species of spikerushes occurred 489 times, yet production of square stem spike-rush was only eight pounds per acre, thus indicating that it is a choice food, the amount available to waterfowl being limited by a low yield.

Wild millet is abundant in many marsh localities, but potential mature yields are often destoryed by late summer and early fall marsh burning. Elimination of marsh burning or burning in winter would prevent this loss.

Although the bulrushes did not constitute a high percentage of the food taken, they did occur 273 times in duck stomachs. The yield of bulrushes is low in poundage. Muskrats are, of course, heavy feeders on certain of the bulrushes and may limit the amount available to waterfowl.

Food habits of geese are in agreement with the analysis made by Glazener (1946) of 22 geese from the Texas coastal rice belt, and 95 geese from the general farming region of the Texas coast. The 31 stomachs examined by the writer were from coastal marshes which are in close proximity to the rice belt, thus affording both rice field and marsh feeding grounds for the geese.

MANAGEMENT SIGNIFICANCE

With our available waterfowl wintering grounds on the decrease due to expanded agricultural developments, drainage, industrialization, and oil exploration, it is becoming increasingly important that steps be taken to determine proper management techniques and to put these techniques into action. While intensive management is most feasible on small acreages, it is felt that certain facts brought out in this paper may be applied to both intensive and extensive management. Such practices as controlled burning and regulated grazing and trapping have been recognized as good management techniques, still they are not practiced nearly as widely as desirable.

Burning of marsh vegetation should be delayed in the fall until the waterfowl food plants such as millet, smartweed, and salt grass have matured seed and these seed have fallen. Most burning at present is usually for one or two reasons: either to create a young succulent growth of marsh grasses for late winter and early spring livestock grazing or to clear the marsh of rank growth to facilitate muskrat trapping. Consequently, little thought has been given to the supply of duck foods. Quite often this burning has improved grazing for geese, but unintentionally. The favorite feeding areas of geese have been these burned areas. Periodic burning of sawgrass, as stated previously, has made the seeds more available to waterfowl. Lynch, O'Neill, and Lay (1947) state that periodic burning of reed cane (*Phragmites communis*) is necessary to prevent it from "taking-over" or crowding out other species of more desirable plants in a marsh.

Insufficient muskrat trapping often results in an overpopulation in a marsh area. This overpopulation, in turn results in "eat-outs" or spots of marsh denuded of vegetation. Such eat-outs are frequently extremely slow in becoming revegetated thus elimin-

ating some plants vitally important in the waterfowl diet. Muskrat trapping should be adjusted to prevent this.

Such valuable food plants as millet, smartweed, pondweeds, and bulrush could be increased by such management as sustained or constant water levels along with the factors pointed out above.

A factor becoming increasingly important at present is marsh drainage. Most of the Texas coast, if not the entire coast, is in drainage districts at present. The objective of drainage work is the improvement of areas above the marsh for additional grazing or cultivation as well as flood control, which certainly are necessary to maintain the required levels of food production for human consumption. Nevertheless, some detrimental effects upon waterfowl and other marsh-loving species of wildlife are going to be an end result. A cooperative study and plan is needed at present while the drainage plans are still in a formulative stage.

It is regrettable that estimates on marsh acreage in Texas 50 years ago are unavailable. If such figures were available they should show conclusively the dire need of proper management of the remaining marsh. It is entirely possible that we are now witnessing the passing of some of the most important winter range for waterfowl on this continent. Effects on the continental population of waterfowl may be readily anticipated.

CONCLUSIONS

1. Waterfowl plant food material yields in the form of seed, stems and leaves, and rootstock were determined for 14 of the better recognized waterfowl food plants on the southeastern Texas Gulf Coast over a period of one year, June 1, 1946 through May 31, 1947. Air-dried yields of seed in pounds per acre were as follows:

Beak rush	910.6
Wild millet	813.3
Smartweed	581.4
Sawgrass	528.5
Pickerelweed	346.0
Leafy three-square	299.0
Rice cut-grass	137.0
Southern bulrush	44.1
Bayonet three-square	24.7
Square stem spike-rush	8.0

Air-dried yields of stems and leaves in pounds per acre were as follows:

Rice cut-grass	6,394.6
Widgeon grass	1,757.2
Glasswort	680.9
Pondweed	600.3
Burhead	535.3

Air-dried yield of rootstock in pounds per acre was as follows:

Leafy three-square corms	9,580.0

Yields of materials in pounds per acre determined while plants were wet are as follows:

Leafy three-square corms	18,990.6
Burhead, entire plant	7,367.9
Pondweed, stems and leaves	5,940.7

2. The average yield of air-dried seed of all species together, per acre was 369.26. Stems and leaves, air-dried, average 10,766.4 pounds per acre.

3. In only one case was there sufficient information to determine effects of water level on productivity. One plot of southern bulrush in a brackish pond with a semi-stabilized water level was the highest producer among the five plots of southern bulrush, thus indicating that stability of water is a limiting factor on productivity.

4. Plant competition was found to be

a limiting factor in seed production of beak-rush and wild millet.

5. It was found that late winter burning retards the growth of sawgrass and accelerates spring growth of smartweed.

6. There has been a change in food habits of ducks based upon a comparison of the analysis by Martin and Uhler (1939) and the present work. At this later date ducks are more dependent upon rice, while the percentages of natural foods has decreased. This shift may be caused in part by the recent increase in rice acreage, which has increased the amount of rice available to ducks.

7. There has been no appreciable change in food habits of geese.

8. There is a definite need for an improved marsh burning program which at present limits the production of waterfowl foods to some extent, especially wild millet.

9. From the information at hand, it appears that there is a need for further study of marsh management practices as related to waterfowl food plant production, with a view to conservation of suitable winter waterfowl areas on the Texas Gulf Coast.

LITERATURE CITED

GLAZENER, W. C. 1946. Food Habits of Wild Geese on the Gulf Coast of Texas. Jour. Wildl. Mgt., **10**: 322–329.

GRISCOM, LUDLOW. 1947. An Ornithologist Looks at the Waterfowl Problem. The Bul. of the Mass. Audubon Soc., **30**: 1–18.

JORDAN, DAVID STARR, and BARTON WARREN EVERMANN. 1900. The Fishes of North and Middle America. Bul. U. S. Nat. Mus. No. 47, Part IV.

LOW, JESSOP B., and FRANK C. BELLROSE, JR. 1944. The Seed and Vegetable Yield of Waterfowl Food Plants in the Illinois River Valley. Jour. Wildl. Mgt., **8**: 7–22.

LYNCH, JOHN J., TED O'NEIL and DANIEL W. LAY. 1947. Management Significance of Damage by Geese and Muskrats to Gulf Coast Marshes. Jour. Wildl. Mgt., **11**: 50–76.

MARTIN, A. C., and F. M. UHLER. 1939. Food of Game Ducks in the United States and Canada. U. S. Dept. Agric. Tech. Bul. 634. 156 pp., illus.

MARTIN, A. C., R. H. GENSCH, and C. P. BROWN. 1946. Alternative Methods in Upland Game Food Analysis. Jour. Wildl. Mgt., **10**: 8–12.

PENFOUND, WILLIAM T., and EDWARD S. HATHAWAY. 1938. Plant Communities in the Marshlands of Southeastern Louisiana. Ecol. Mono., **8**: 1–56.

PRATT, HENRY SHERRING. 1935. A Manual of the Common Invertebrate Animals. 854 pp., illus.

Accepted for Publication May 17, 1950.

WEIGHTS AND MEASUREMENTS OF ROCKY MOUNTAIN ELK[1]

Don C. Quimby and Donald E. Johnson
Montana Agricultural Experiment Station, Bozeman, Montana

During December 1949, the writers examined several Rocky Mountain elk, *Cervus canadensis nelsoni*, taken from the northern herd of Yellowstone National Park by park officials in a management operation. We secured standard measurements of 85. Whole weights were obtained for 22. Our data were gathered incidental to the management operation making it difficult to secure complete information for all specimens. Since accurate measurements and weights of this species are not readily obtainable, we thought the data worth recording. Furthermore, the weighing of the animals by parts provided some information on the relationship between whole weights and the weights of partially or completely dressed carcasses.

The animals were shot and eviscerated in the field by park employees and later removed to a central location for final dressing and quartering. Measurements were taken at the kill site. Total length and hind foot measurements were taken with a steel tape; tail and ear measurements with a ruler.

The visceral weights were also secured at the kill site. The viscera were placed in a large pan equipped with a rope for weighing on a steelyard suspended from a tripod. The tripod, designed by the junior author, was especially helpful in securing weights under field conditions. Three 3/4 inch pipes, 4 feet long, with a T-hinge welded to one end, were used for legs. The three T-hinges were joined by welds making a substantial, collapsible tripod which was easily transported. One of the legs was equipped with a hook 18 inches from the top for support of the steelyard. When weighing, one man elevated this leg to about shoulder height while the second manipulated the weights on the steelyard.

The carcasses were tagged in the field making it possible to identify individuals at the central butchering point. Here we weighed the remaining parts on a Howe platform scale. As an animal was butchered we weighed in turn the feet, head, skin, and quarters. In two instances the head and feet were weighed together.

Our thanks are extended to the following: Yellowstone National Park officials for permitting us to secure data in the park and granting mess and bunkhouse facilities; Walter Kittams, park biologist, for aiding us on several occasions, especially in determining age classes; Joe Hinkle, Conservation Officer of the Montana Fish and Game Department, whose excellent cooperation made it possible for us to secure dressed weights; Charles B. Brotherton, of Montana State College, for advice and help with the statistical analysis.

Measurements

The means and ranges of standard measurements are given in Table 1

[1] Contribution from Montana State College, Agricultural Experiment Station. Project No. MS844, Paper No. 229, Journal Series.

TABLE 1.—STANDARD MEASUREMENTS IN INCHES OF EIGHTY-FIVE ROCKY MOUNTAIN ELK

Age Class	Sex	No.	Total Length Mean	Total Length Range	Tail Mean	Tail Range	Hind Foot Mean	Hind Foot Range	Ear Mean	Ear Range
Calf	M	3	68.92	67.50–71.25	5.08	4.75–5.50	22.25	22.00–22.50	7.17	7.00–7.50
Calf	F	6	67.25	61.25–75.75	4.62	4.00–5.25	22.42	20.25–24.00	7.12	6.75–7.50
Yearling	M	2	79.00	75.00–83.00	5.00	5.00–5.00	24.25	24.00–24.50	8.12	8.00–8.25
Yearling	F	4	78.87	77.00–81.50	4.56	3.50–5.00	23.81	22.50–24.50	7.62	7.00–8.00
2-Year Old	M									
2-Year Old	F	3	85.33	85.00–86.00	5.58	4.75–6.50	25.04	24.75–25.37	7.96	7.75–8.25
3-Year Old or Older	M	12	95.40	91.00–99.00	5.64	5.25–6.25	26.35	25.25–27.25	8.33	7.50–9.00
3-Year Old or Older	F	55	89.44	82.00–97.75	5.37	4.00–7.00	24.89	23.75–26.37	8.00	7.25–8.75

according to sex and age classes. The age classes as given are "long" inasmuch as the animals were somewhat older than indicated. Thus the calves were about 7 months old, the yearlings about 19 months, etc. Schwartz and Mitchell (1945) give average measurements of the Roosevelt elk, *Cervus canadensis roosevelti*, from the Olympic Peninsula, Washington as follows: 5 year old male; 98–4–28–8, 4 year old female; 92–4–26.5–8, 9 month old male; 72–3–23.5–8. Although they do not say how many they measured, their data suggest the Roosevelt elk has a greater total length and larger hind foot but a shorter tail than the Rocky Mountain elk. The ear lengths of the two seem about the same.

WHOLE WEIGHTS

"Whole weights" for 22 animals were obtained by totaling the parts weighed (Table 2). These weights do not repre-

TABLE 2.—WEIGHTS AND WEIGHT RELATIONSHIPS OF TWENTY-TWO ROCKY MOUNTAIN ELK. (POUNDS).

Age Class	Sex	Visceral Weight	Hog-dressed Weight	Dressed Weight	Whole Weight	Percent of Total Weight: Visceral	Hog-dressed Carcass	Dressed Carcass
Calf	F	85	166.5		251.5	34	66	
3-Years Old or Older	M	226	497.5	396	723.5	31	69	55
		220	457.0	365	677.0	32	68	54
		225	471.75	361	696.75	32	68	52
		230	463.75	355	693.75	33	67	51
		267	555.0	433.5	822.0	32	68	53
		275	523.25	407.0	798.25	34	66	51
		215	458.25	354.0	673.25	32	68	53
		261	555.5	439.5	816.5	32	68	54
		241	497.5	387.5	738.5	33	67	52
		209	448.0	352.0	657.0	32	68	54
	Mean	236.9	492.75	385.05	729.65	32	68	53
	F	167.5	371.5	311.5	539	31	69	58
		165	379.5	312	544.5	30	70	57
		198	445	365	643	31	69	57
		192	391.5	325.5	583.5	33	67	56
		169	376.5	309.5	545.5	31	69	57
		174	371	310	545	32	68	57
		175	381.5	310	556.5	31	69	56
		224.5	323	260	547.5	41	59	47
		185	334.5	278	519.5	36	64	53
		202	406.5	340.5	608.5	33	67	56
		178	366.5	305	544.5	33	67	56
	Mean	184.55	377	311.55	561.55	33	67	55

sent actual total weights because of blood loss due to shot wounds and bleeding induced by thrusting a knife into the thoracic cavity or cutting the animal's throat soon after it was shot. Not all of the blood was lost. The viscera usually contained considerable amounts when weighed.

Ten mature bulls (3 or more years old) averaged 729.65 pounds. Eleven mature cows averaged 561.55 pounds. A female calf weighed 251.5 pounds. The maximum weights recorded, 822 pounds for a male and 643 pounds for a cow, probably approach the upper weight classes for this animal. Concerning the Roosevelt elk, Schwartz and Mitchell (op. cit.) state: "A mature bull elk in prime condition will weigh 700 to 1,000 pounds and an average cow in good shape between 400 and 700 pounds. Two adult females weighed 405 and 425 pounds, respectively; both were in poor condition, and unquestionably from 100 to 200 pounds under maximum weight."

WEIGHT RELATIONSHIPS

Hog-dressed to Total Weight. Visceral weight, hog-dressed weight, dressed weight, and whole weight are recorded for individual animals in Table 2. The partial weights are also expressed as percent of whole weights. Visceral weight includes all internal organs and the external genitalia as removed at the kill site. Hog-dressed weight is whole weight less viscera. Dressed weight is whole weight less viscera, head, feet, and skin.

An examination of these data reveals a somewhat definite and constant relationship between the various partial weights and whole weights. Thus the viscera average about one third (males 32, females 33%) and hog-dressed carcasses about two thirds (males 68, females 67%) of total weights. Only one, a cow, deviates markedly from these figures (viscera constituting 41% of total weight). We are unable to account for this large discrepancy. Perhaps this animal was diseased or in poor condition. If so, it escaped our attention. Hamerstrom and Camburn (1950) present evidence which make such an assumption seem reasonable. They state, 'It is probably safe to say that within years, as between years, the hog-dressed weight of deer in good condition tends to be greater than that of "poor" animals of the same whole weight.'

The data of this small sample suggest the possibility of predicting whole weights of elk when hog-dressed weights are known. Several methods for calculating whole from hog-dressed weights based on weight relations of various Cervidae have been described. Hornaday (1935) weighed a single antlered elk in 1903 taken from the New York Zoological Park. From this observation, he offered the following as a basis for calculating the weight of deer. "The dressed weight being given, in pounds, add to it five ciphers, divide by 78612, and the result will be the live weight, in pounds." Hunter (1947) from his study of mule deer in Colorado recommends for "all species of Deer": "To obtain live weight add 1/4 to hog-dressed weight." Hamerstrom and Camburn (op. cit.) made a very careful study of the weight relationships of 131 white-tailed deer from Michigan. Their data are expressed as regression of whole weight on hog-dressed weight which

yields a formula for predicting whole weight when only hog-dressed weight is known. Our data (Table 2) suggest another method to apply to elk—"add 1/2 to hog-dressed weight."

Our small sample of 22 known weight elk supplies some basis for testing the accuracy of each of these methods so far as elk are concerned. The calculated whole weights and their deviations from known whole weights are given in Table 3. The calculated equation for the regression of whole weight (Y) on hog-dressed weight (X) for the 22 elk is: $Y = 22.113209 + 1.432584X$.

The results obtained by the four methods are quite variable (Table 3). On the average, the calculated weights obtained by the regression formula more nearly approached true weights; the average deviation from true weight being 11.63 pounds with extreme "misses" of −.2 and −62.66 pounds. The calculated weights of the method of present study were not appreciably different, however (average deviation from true weight 14.92 pounds; extreme "misses" +1.25 and −63.00 pounds). The method of Hunter and the Hornaday formula gave calculated weights which deviated considerably more from true weights than did the calculated weights obtained by the two other methods; method of Hunter −98.81 pounds (−43.38 to −144.19 pounds), Hornaday formula −89.54 pounds (−39.7 to −136.62 pounds). The calculated weights of each of these methods were consistently less than true weights (Table 3).

TABLE 3.—CALCULATED WHOLE WEIGHTS FROM HOG-DRESSED WEIGHTS OF TWENTY-TWO ROCKY MOUNTAIN ELK. (DATA FROM TABLE 2)

Hornaday Formula		Method of Hunter		Regression Formula		Present Study	
Weight	Deviation[1]	Weight	Deviation[1]	Weight	Deviation[1]	Weight	Deviation[1]
211.8	− 39.70	208.12	− 43.38	260.64	+ 9.14	249.75	− 1.75
632.85	− 90.65	621.87	−101.63	734.82	+ 11.32	746.25	+ 22.75
581.34	− 95.66	571.25	−105.75	676.80	− .20	685.50	+ 8.50
600.1	− 96.65	589.69	−107.06	697.93	+ 1.18	707.62	+ 10.87
589.92	−103.83	579.69	−114.06	686.47	− 7.28	695.62	+ 1.87
706	−116.00	693.75	−128.25	817.20	− 4.80	832.50	+ 10.50
665.61	−132.64	654.06	−144.19	771.71	− 26.54	784.87	− 13.38
582.93	− 90.32	572.81	−100.44	678.59	+ 5.34	687.37	+ 14.12
706.63	−109.87	694.37	−122.13	817.91	+ 1.41	833.25	+ 16.75
632.85	−105.65	621.87	−116.63	734.82	− 3.68	746.25	+ 7.75
569.89	− 87.11	560.00	− 97.00	663.91	+ 6.91	672.00	+ 15.00
472.57	− 66.43	464.37	− 74.63	554.32	+ 15.32	557.25	+ 18.25
482.75	− 61.75	474.37	− 70.13	565.78	+ 21.28	569.25	+ 24.75
566.07	− 76.93	556.25	− 86.75	659.61	+ 16.61	667.50	+ 24.50
498.01	− 85.49	489.37	− 94.13	582.97	− .53	587.25	+ 3.75
478.93	− 66.57	470.62	− 74.88	561.48	+ 15.98	564.75	+ 19.25
471.94	− 73.06	463.75	− 81.25	553.60	+ 8.60	556.50	+ 11.50
485.29	− 71.21	476.87	− 79.63	568.64	+ 12.14	572.25	+ 15.75
410.88	−136.62	403.75	−143.75	484.84	− 62.66	484.50	− 63.00
425.51	− 93.99	418.12	−101.38	501.31	− 18.19	501.75	− 17.75
517.1	− 91.40	508.12	−100.38	604.46	− 4.04	609.75	+ 1.25
466.21	− 78.29	458.12	− 86.38	547.15	+ 2.65	549.75	+ 5.25
Mean	− 89.54		− 98.81		00		+ 6.20
					11.63[2]		14.92[2]

[1] From actual total weight.

[2] Disregarding signs.

Do any of the methods give sufficiently accurate results to justify their use in predicting whole weights of the 22 elk if only hog-dressed weights were known? The answer seemingly would be governed by the degree of accuracy desired. Obviously the method of Hunter and the Hornaday formula did not produce very satisfactory results. The two other methods gave considerably more accurate predictions. Only one calculated weight, the "odd" cow, determined by either of these methods deviated more than 26.54 pounds from true weight. To us, this does not seem to be too unreasonable for animals as large as elk. The data would, of course, be more easily evaluated if we knew how often one would encounter an elk that deviated as far from normal as did the "odd" cow.

Although the regression formula gave the most accurate predictions, it does not seem to us to be sufficiently more reliable than the method of present study to greatly overshadow the simplicity of the latter for practical purposes. The fact that the contents of the paunch might greatly influence the visceral weight of an elk would seemingly discredit the possibility of an exacting method. Furthermore, Hamerstrom and Camburn (op. cit.) found that the hog-dressed weights of white-tailed deer in good condition were heavier than that of "poor" animals of the same whole weights.

Dressed Weight to Whole Weight. The dressed weights of the males averaged 53 percent of total weights (51 to 55). Those of the females averaged higher, 55 percent (47 to 58). Excluding the "odd" cow, the range was 53 to 58 percent (Table 2). White-tailed deer seemingly "dress out" at about the same percentage. Hamerstrom and Camburn (op. cit.) recorded whole weights and fully dressed weights of 24. The dressed weights appear to average about 52 percent of whole weights.

Weights of Heads, Feet, and Skins. Weights of heads, feet and skins are given in Table 4. The feet were removed at the metapodial joint; the heads in the vicinity of the first cervical vertebra. Except for the portions covering the head and feet, the skins were complete. Varying amounts of flesh and connective tissue remained on them.

SUMMARY

Standard measurements of Rocky Mountain elk of both sexes and various age classes are given. Visceral weights, hog-dressed weights, dressed weights, whole weights, and weights of heads, feet, and skins are recorded. Visceral,

TABLE 4.—WEIGHTS OF HEADS, FEET, AND SKINS OF ROCKY MOUNTAIN ELK

	Head			Feet			Skin		
	No.	Mean	Range	No.	Mean	Range	No.	Mean	Range
Females 3-Years Old or Older	9	24.11	19.5–29	9	12.67	11.5–13.5	11	28.95	24.5–37.5
Males 3-Years Old or Older	10	51.25	42–60.5	10	15.55	14.25–17	10	40.9	30.5–46

hog-dressed, and dressed weights are expressed as percent of whole weights. The somewhat definite and constant relationship existing between hog-dressed and whole weights provided a basis for predicting whole weights from hog-dressed weights. Various methods of making this prediction are evaluated on the basis of a small sample of 22 elk.

LITERATURE CITED

HAMERSTROM, F. N. JR. and F. LAWRENCE CAMBURN. 1950. Weight relationships in the George Reserve deer herd. Jour. Mamm., 31(1): 5–17.

HORNADAY, WILLIAM T. 1935. Hornaday's American natural history. Charles Scribner's Sons, New York.

HUNTER, GILBERT N. (1947) Physical characteristics of Colorado mule deer in relation to their age class. Colo. Game and Fish Dept., Denver.

SCHWARTZ, JOHN E. II and GLEN E. MITCHELL. 1945. The Roosevelt elk on the Olympic Peninsula, Washington. Jour. Wildl. Mgt., 9(4): 295–319.

Accepted for Publication May 26, 1950.

SIZE OF BAIT FOR POCKET GOPHER CONTROL

Milton A. Miller and Walter E. Howard
Division of Zoology, University of California, Davis, California

There is a common belief that poison baits, to be most effective for controlling pocket gophers, should be large enough to prevent the animal from stowing them in its fur-lined cheek pouches without first gnawing them down to proper size: whereupon presumably, a lethal dose of poison would get into its mouth and be absorbed. Small baits, according to this theory, might be pouched with impunity because the cheek pouches of a gopher, unlike those of ground squirrels and some other rodents, are impermeable to poison. Since pouched materials are frequently carried away and stored underground by the gopher, there is a possibility that such caches would be forgotten, spoil or lose their potency. This theory has been invoked to account for the relative ineffectiveness of poisoned grain, raisins and certain other baits of small size in controlling pocket gophers. On the basis of the foregoing concepts, many control bulletins specify the sizes for vegetable baits commonly advising that they be cut into pieces about 1/2 x 1/2 x 1 1/2 inches. Such bait is easy to insert into a gopher's runway, but too large for the rodent to pouch without cutting. Obviously, poison baits that are eaten soon after placement are not involved here.

To test this theory critically, comparative field tests were conducted on farm lands at Davis, California, to determine the effect of bait size on kill of gophers (*Thomomys bottae navus*). Additional observations were made at the San Joaquin Experimental Range, O'Neals, California, on the handling of baits by gophers (*T. b. mewa*) in the field and also confined in dirt-filled cages, either glass-topped or glass-sided, in the laboratory. These studies are a part of a comprehensive investi-

gation on poison baits for pocket gophers on which further results will be published later.

FIELD TESTS ON POISON BAITS OF VARIOUS SIZES

The "open-hole test" was used to evaluate the results with baits of different sizes under field conditions. This test is based on two traits of gophers: (1) they are remarkably solitary, only one individual normally inhabiting a burrow system, and (2) they will almost always plug any opening made into a runway. The test procedure is as follows. Known amounts of baits are inserted into gopher burrows (conveniently through holes made by the probe used in locating the runways), the bait sites are marked above ground by flags, and a day or two later they are dug open with a shovel. The amount of bait taken by the gopher is recorded for later use in estimating acceptance, and the number of tunnels opening into each excavated bait site is noted. On the following day, the bait sites are revisited to determine whether or not a kill was made. If an exposed tunnel remains open, it is assumed that the gopher was killed; but if one or more such holes are plugged, the occupant presumably is still alive. Occasionally, a dead gopher is found at the bait site as direct evidence of kill, but usually reliance is placed on circumstantial evidence, the absence of plugging. That a live gopher practically always will plug an opened bait site has been demonstrated by open-hole tests using non-toxic baits. Of 138 burrows in which unpoisoned baits were offered and taken, only three (about 2.2%) remained open. Failure to plug in these few exceptional instances may be due to natural mortality, abandonment of the burrow, or other reasons.

The open-hole test has certain limitations. At times, more than one gopher may inhabit a burrow system, as perhaps during breeding season. Hence, if all residents of a burrow are not killed in a test, the opened bait site will be plugged by a survivor. Also, it is possible that another gopher might move into the burrow system immediately after death of the original occupant and plug any openings. In both such instances, it would seem that no kill was made; and thus any mortality estimate based on the percentage of bait sites remaining open would tend to be lower than that actually obtained. A correction might be applied to such kill percentages if the incidence of plural occupancy of burrows at the time of testing could be fairly estimated. A better way of circumventing the difficulty, however, is to evaluate the effectiveness of treatments on a *relative* rather than on an *absolute* basis. This involves either comparison of performance of various treatments or formulations tested at the same time, or rating them against a standard or toxic control the performance of which, under similar conditions, is known. Concurrent testing with adequately large samples cancels out extraneous variables so that any differences in performance can be attributed to differences in formulation or treatment.

For practical purposes, experience has shown that the percentage of burrow systems that remain open after treatment with a poison bait is a reliable index of its effectiveness. This percentage will hereafter be called "field-

kill" since it involves acceptance as well as toxicity and thus measures the over-all operational efficiency of the bait. The field-kill percentage and its standard error are calculated by the formula,

$$\text{Field-kill percent} = 100\left[\frac{n}{N} \pm \sqrt{\frac{p(1-p)}{N}}\right]$$

where n = the number of systems remaining open after poisoning, N = the total number of inhabited systems successfully treated (regardless of the amount of bait taken), and $p = \frac{n}{N}$, the field kill expressed as a decimal.

In preparing baits for the field tests, carrots and prunes were cut into pieces of graded size and evenly dusted with strychnine alkaloid. The carrots were cut into three sizes: (1) cylinders 1 1/4 inches in length and 5/8 inches in diameter, hereafter called the "standard piece", (2) the standard piece cut into eight equal parts, and (3) the standard piece cut into 20 equal parts, approximately 1/4-inch cubes. The standard piece averages about seven grams and the other two sizes 0.87 gm. and 0.35 gm., respectively. Two sizes of prune baits were used: (1) whole dried prunes averaging 6.5 grams in weight including pit, and (2) prunes cut into small pieces, each roughly the size of a large raisin and weighing about 0.33 gm. Twenty such pieces were roughly equivalent in weight to one whole prune.

In dusting the baits with the strychnine alkaloid powder, the weight ratio of poison-to-bait was kept constant at 1:280 for carrots and 1:260 for prunes. In other words, each standard piece was coated with 25 milligrams of strychnine and the smaller pieces each received an amount of poison proportional to its weight. The strychnine was dusted evenly over the baits, a little at a time, which were rolled and shaken between dustings in a mixing jar until each piece was uniformly coated.

Five series of gopher burrows were treated, each with a different size of bait. The experiment was so designed that each burrow received the same total weight of bait, and hence the same amount of poison, so that the only variable was size of bait. Two standard pieces or the equivalent amount of the smaller baits were inserted into each burrow. Hence, each gopher was offered 50 mg. of strychnine on either 14 grams of carrots or 13 grams of prunes.

Field-kill percentages for the different-sized baits were computed from the open-hole test data and compared statistically to determine if there were any significant differences in kill associated with size of bait. Acceptance percentages (amount of bait taken/amount offered) for the various sized baits were also calculated and compared.

The results of these tests are given in Table 1. Also tabulated there for comparison are data obtained in field trials with other baits of small size, namely, strychnine-coated raisins and grain. Although the latter experiments were made at different times, the procedures, dosages of poison and poison-to-bait ratios were the same for all tests so that the results are comparable.

With carrot baits, it appears at first glance as if there was an increase in kill with increase in size of bait, from 82% kill with the smallest bait to 91% kill with the largest, but this difference is not statistically significant. The 91% kill obtained with the largest-sized

TABLE 1—FIELD TESTS ON THE ROLE OF BAIT SIZE IN THE EFFECTIVENESS OF VARIOUS STRYCHNINE-COATED BAITS FOR POCKET GOPHERS, WITH DOSAGE CONSTANT AT 50 MG. PER BURROW AND POISON-TO-BAIT RATIOS CONSTANT AT 1:280 (CARROTS, RAISINS, AND OAT GROATS) OR 1:260 (PRUNES) BY WEIGHT.

Bait	Size of bait, grams per piece	Amount of bait offered per gopher		No. of burrows baited	Acceptance % ± S.E.	Field-kill % ± S.E.
		No. of pieces	Weight in grams			
	gm.		gm.		%	%
Carrots	7.0	2	14	32	93 ± 4.5	91 ± 5.0
				157	92 ± 2.1	80 ± 3.1
	0.87	16	14	51	88 ± 4.5	84 ± 5.1
	0.35	40	14	50	90 ± 4.2	82 ± 5.1
Prunes	6.5	2	13	18	97 ± 4.0	67 ± 11.0
	0.33	40	13	22	99 ± 2.1	68 ± 9.9
Raisins	0.35	40	14	41	91 ± 4.4	36 ± 7.4
Oat groats (whole)	0.022	720	14	53	85 ± 4.9	40 ± 6.7

carrot baits in this experiment is somewhat higher than normal for this formula which, as shown in Table 1, ordinarily averages about 80%. A Chi-square test on the carrot bait data of the present experiment further shows a definite lack of association between kill and size of bait ($P = 0.98$). It is probable that other root vegetables cut in comparable sizes would give results similar to those here reported for carrots, since the same kills (within the experimental error) have been obtained with standard-sized baits for all root vegetables thus far tested.

With prune baits also, there was no significant difference in kill associated with size of bait. Since experiments have also shown that various dried fruit baits of standard size give about the same kills as whole prunes, it is likely that they too could be cut into smaller sizes, if so desired, without affecting the kill.

Taken collectively, carrot baits gave significantly higher kills than prunes, and these in turn were significantly more lethal than raisins or grain although the absolute dosage of strychnine was the same for all. Clearly, therefore, the nature of the bait material, rather than its size, is of paramount importance in dealing with strychnine-coated baits. There seems to be a direct correlation between perishability of baits and their effectiveness. A possible explanation is that the more permanent baits are stored to a greater extent by the gopher with consequent loss of toxicity, but palatability may also be a factor.

Unfortunately, the method used in these tests gives little or no information as to the reason for these differences. Acceptance percentages were practically the same for all baits regardless of kind or size, but these figures indicate only the amount removed from the bait site by the gopher and give no clue as to how this was accomplished or whether the baits were eaten or stored. Furthermore, field-kill percentages, as estimated here, merely indicate the mortality during the testing period, a matter of days, and yield no information on the toxicity of baits eaten after storage. Thus, grain baits and raisins might be more effective than indicated in these experiments. Nevertheless, it

seems probable that baits eaten more or less immediately would be better than those stored by gophers. Laboratory observations, to be cited in the following section of this paper, will answer some of these questions.

Handling of various-sized clean baits by gophers

A female pocket gopher (*Thomomys bottae mewa*) was housed in a dirt-filled laboratory cage 56 feet long, from 4 3/4 to 19 inches wide, and 3 1/4 inches deep. The soil rested on a concrete slab and the cage was covered with plate glass. Between observations the glass was covered with canvas to exclude light.

The animal was fed 46 carrots at different times, but none was cut into smaller segments for carrying in the cheek pouches. If not eaten, each was cached entire. On one occasion, 22 carrots (average weight about 40 grams) were placed just outside one entrance to a burrow in the above-mentioned cage, and 21 were carried into the burrow system during the second night. All but one were cached 24 to 28 feet from the entrance, the one exception being carried only 20 feet. The gopher cut the tops from all carrots before storing them.

In another experiment, mature carrots were planted artificially in a glass-sided cage (3 inches wide by 24 inches deep) filled with soil and housing a male pocket gopher. The animal was observed through the glass as it removed the carrots. Any carrot in loose soil was easily pulled into the burrow. The animal was unable to travel forward with such a large object but promptly turned about and ran backwards for nearly four feet dragging the carrot to its nest. When the earth was packed firmly around the carrots, the gopher cut and took as large a piece as it was able to get free at one time. It then carried the fragment to its nest and returned to dig for more.

The preceding two tests were conducted under simulated natural conditions, but the question arises, would gophers in the field deal with whole carrots in a manner similar to that of caged animals in the laboratory? Accordingly, entire carrots were inserted into gopher burrows in the field. When the burrows were excavated the following day, no carrots could be found. This was not surprising because food caches in the laboratory were almost always in a sealed compartment that was plugged from the main runways for many inches. Five feet of silk thread was then tied to entire carrots as a possible aid in locating them in caches when the burrows were excavated; yet the thread could not be found in three such tests. Next a spool of fine copper wire was attached to a carrot and suspended on an axle so that the wire could easily be pulled into the burrow. Nine feet of wire was drawn into the burrow before the animal apparently cut the carrot free.

The foregoing experiments indicate that gophers do not cut and pouch entire carrots, as has been supposed, but instead they carry or drag them along their burrows for considerable distances to nests or caching chambers. Whole carrots may at times be trimmed or cut into large chunks for ease in handling, but they apparently are not cut into small pieces for pouching.

To observe the handling of smaller bait pieces, carrots were cut into the

same three sizes as used in the field tests with poison baits and offered to a caged female as indicated in Table 2. Entire carrots were also used for comparison. All carrots, regardless of size, were taken to the nest, but the way in which they were transported depended on the size. The two smaller sizes were carried in the cheek pouches. The standard pieces (1/2 x 1/2 x 1 1/2 inches) were carried in her mouth as she travelled sometimes forward and sometimes backward to the nest. Entire carrots were dragged in the manner previously described. She had difficulty dragging one carrot through her tunnels, but when it became lodged, she enlarged her burrow! At one place where the carrot became wedged, she cut a small section from its end. Again, it is clear that large pieces or entire carrots are not cut to pouchable size.

TABLE 2—RATE OF CONSUMPTION OF CARROTS BY A CAGED POCKET GOPHER IN RELATION TO SIZE OF PIECE

Size of piece	Number of pieces offered	Consumption as indicated by number of pieces remaining after:			
		7 days	9 days	12 days	14 days
Entire carrot (medium size)	2	2*	2	1 2/3	(1/2 + 1/2)
Standard bait (1 1/4″ long x 5/8″ diam.)	5	3 1/3	2	1/2	0
1/8 standard bait	32	9	6	0	0
1/20 standard bait	60	3	0	0	0

* Few bites taken from carrot.

The rate of consumption of the carrots (Table 2) was definitely associated with the size of piece, the smaller ones being eaten first and the entire carrots saved until last. The smaller pieces, with relatively greater exposed surface, would be more apt to spoil or mold than the larger ones, especially the entire carrots which in addition are protected by the skin (periderm). Whether the gopher senses which baits are more perishable than others and acts accordingly is, of course, a matter of conjecture.

The observations on bait handling also refute the idea that baits must be too large for the gopher to pouch without trimming them, because the animals readily transport large items without pouching. Possibly the gopher instinctively realizes that the less such foods are cut, the better they will keep and that small pieces are apt to dry out or spoil much quicker than large ones. Small pieces of perishable baits might even have at least a theoretical advantage over large pieces in that the small ones would be eaten more or less immediately without any attempt at storage. In any event, the observational and experimental data herein presented definitely show that, within practical limits, vegetable and probably dried fruit baits may be diced or cut to whatever size the operator finds most convenient.

SUMMARY

Any size of bait within practical limits, may be used in poisoning pocket gophers provided the right kind of bait and adequate dosages of poison are used. This conclusion is based on quanti-

tative field tests using strychnine-coated gopher baits of various sizes and kinds, and observations on how caged and field gophers handled different-sized unpoisoned baits. The data presented refute the theory that relatively large baits—too large for the gopher to pouch without cutting—are necessary for successful poisoning with strychnine-coated carrots, prunes, and probably with other vegetable and dried fruit baits. These materials may be cut to any convenient size without loss of effectiveness. Moreover, pocket gophers do not cut large pieces of bait or even whole carrots in order to pouch them, but transport them entire. The fact that strychnine-coated carrots of graded sizes were significantly more lethal than prune baits of comparable size and dosage, and that both carrot and prune baits were much more potent than grain and raisin baits which were relatively ineffective clearly shows that the kind of bait, rather than its size, is of paramount importance in strychnine baits for gophers.

NOTES ON THE ARTIFICIAL PROPAGATION OF WOOD DUCK

Harold C. Hanson
Illinois Natural History Survey

In 1938, the Illinois Natural History Survey initiated a program of research on the wood duck (*Aix sponsa*). Since one of the objectives of this project was to test the homing instincts of pen-reared wood ducks, a method of propagating this species artificially was sought. Attempts by Survey personnel to rear wood ducks by artificial means at the Bright Land Farm near Barrington, Illinois, in 1941 and 1942 were only partially successful. In 1944, a third attempt, carried out at the Quiver Creek Laboratory of the Illinois Natural History Survey near Havana, Illinois was more satisfactory. The procedure and outcome of this last rearing attempt is herewith recorded because of the paucity of published techniques of waterfowl propagation (Greenberg 1949) and the now proven feasibility of releasing pen-reared wood ducks for the purpose of restocking depleted areas. (See McCabe, 1947, for results of the release of pen-reared wood ducks made in Wisconsin by the Natural History Survey.)

EGG COLLECTING

The eggs used in this study were collected from nest boxes previously set out in the woodlands of the Illinois River valley. Collections were made several times a week, beginning with the last ten days in April and continuing through the first three weeks in June. During this period 467 eggs were gathered.

INCUBATION

At the time of collection, the eggs were in all stages of incubation, es-

pecially toward the end of the collection period. The majority of the clutches, however, had been incubated less than a week before being placed in the incubator.

The incubation phase of the project was carried out at the Bonnett Turkey Hatchery at Havana, Illinois. Of necessity, the wood duck eggs underwent conditions of incubation recommended for turkey eggs. The temperature was held at 99° F. throughout the period of incubation and the relative humidity was maintained at 80%, excepting that the latter was raised to 90% when the turkey hatch was coming off. The turkey hatch and the corresponding change in humidity coincided only occasionally with the hatch of the wood duck eggs.

A day prior to the hatch of the turkey eggs, the incubators were fumigated by hanging up cloth soaked in formalin. All early hatched turkey poults and those wood ducks which hatched on these days survived such fumigation without suffering any apparent harm.

HATCHING AND REARING SUCCESS

Data for all eggs placed in the incubators are incomplete because 50 eggs (almost all of which contained dead embryos) remaining in the trays after the hatch were inadvertently discarded at the hatchery before they could be examined. Of the 467 eggs brought to the hatchery undamaged, 297 (63.6 per cent) hatched successfully. Of the unhatched eggs which could be accounted for, 40 (8.6 per cent) were infertile and 80 (17.1 per cent) contained dead embryos. The death of most embryos occurred during the late stages of incubation; failure to emerge at time of pipping also accounted for a fair number of deaths.

Out of a total of 297 ducklings taken from the incubator, 242 were successfully reared to an average age of 5 to 6 weeks, a rearing success of 81 per cent. At this age they were transported to points in Wisconsin, Indiana and Illinois for an additional 1 to 3 weeks of captivity prior to release in the wild.

REARING EQUIPMENT

Two Wisconsin-type pheasant brooder houses were used in conjunction with specially built exercising pens. The brooders (Plate 3 A) are small house-like structures with an enclosed area approximately five feet square and, including small wire "sun porches", they have an overall length of six and one-half feet. The interior of each unit is divided into two well insulated sections, containing thermostatically controlled electric heaters which cover one-half of the floor space. These heaters were regulated so as to maintain a temperature of 90° F. in the five inch air space beneath them. As the ducklings grew older, they quickly learned to choose a cooler portion of the brooder, so little attempt was made to reduce temperatures progressively under the heating unit. In fact, dirt in the thermostatic unit sometimes kept the heaters operating constantly with no visible ill effects on the ducklings. Sawdust was used as litter in the brooders as no commercial litters were available at the time.

Each runway or exercising pen was 4 feet wide, 12 feet long and 2 feet high. Two such pens constituted a single unit to correspond with the two sections in the brooder house (Plate

3 A and B). Access to the pens was provided by two doors, four by six feet, which opened the pens from the top. The two doors nearest the brooder house were covered with celoglass to keep rain off the food pans as well as to provide additional shelter. Flooring of the pens consisted of wood frames covered with half-inch wire cloth, and each pen was provided with a small water tank which could be easily lifted out and emptied. These tanks, 16 inches wide, 24 inches long and 7 inches deep (Plate 3 C), had a 15-quart capacity. To avoid wastage and accumulation of food under the wire flooring, each feeding tray was set over a galvanized metal pan 16 inches wide, 24 inches long and 1½ inches deep. Spilled food could then be readily salvaged or discarded, depending upon its condition.

FEEDING AND CARE

A brooder and runway unit accommodated 50 ducklings without serious crowding, although in one of the units 63 ducklings were raised to five weeks of age, or nearly to adult size. Thus, an average of slightly less than one square foot of floor space was generally available to each duckling. This figure approaches the density of 1 bird per 0.6 to 0.7 square feet of floor space commonly recommended for battery-reared chickens.

The majority of the early ducklings were first fed on chicken eggs that had been boiled at least 15 minutes to kill any paratyphoid organisms they might contain. When insects were available, as explained later, or when a few older ducklings were kept in the pen with newly hatched birds to help stimulate the latter to eat, commercial turkey starting mash in pellet form was the only food given. Lettuce was fed at the outset but was soon discontinued when it seemed evident that the ducklings thrived equally well without it. The great majority of the ducklings were reared without access to grit for similar reasons.

For the first few days the newly hatched ducklings were confined to the brooder houses and "sun porches". The brooders and "sun porches", and later the pens, were lighted at night in order that the ducklings could feed throughout the 24-hour period. When it was observed that night lighting was attracting a bounteous supply of insects, an insect electrocuting device was supplied, but, as this apparatus was found to singe the insects, it was abandoned and a 150-watt bulb with reflector was used in each pen.

The ducklings secured endless exercise in pursuit of live insects attracted to the lights, and, in all likelihood, the insect consumption by each duckling was many times greater than that of individuals in the wild. (Plate 3 B and D). The large numbers of insects eaten are believed to have contributed greatly to the physical condition and the health of the ducklings reared (Plate 4 A-D). Turkey starting mash (contents: protein, 26.0 per cent; fat, 3.5 per cent; Vitamin A, 2,000 units; Vitamin D feeding oil, 400 units per gram; and D activated animal-sterol) in small pellet form was the only food provided. Only a few ducklings were weighed because of the disturbance it caused; the average weight of four ducklings averaging 40 days (34–42) in age, was 1.0 pounds.

Food consumption, exclusive of insects, could only be estimated since at

PLATE 3. Rearing wood ducks. A. Brooder house and exercising pens. B. Night-lighted pen. C. Feeders and watering tank. D. Wood ducks catching insects attracted to light.

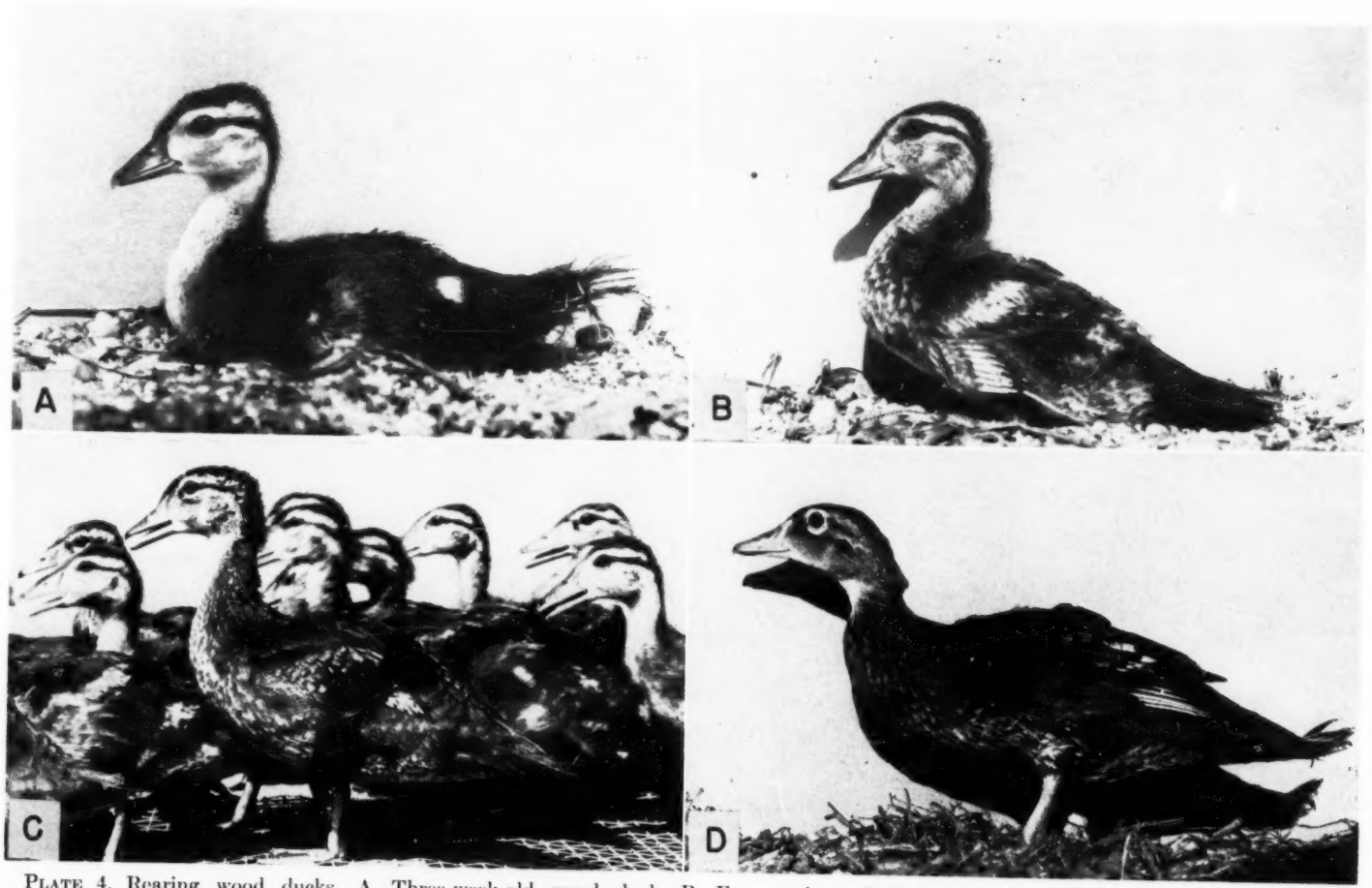

PLATE 4. Rearing wood ducks. A. Three-week-old wood duck. B. Four and one-half weeks old. C. Center duck (male), five and one-half weeks old (38–40 days). D. Female wood duck 54 days old.

least 15 per cent of the mash fed was wasted. Actual consumption of food by the 242 ducks brought to an average age of 5½ weeks is estimated to be about 750 pounds. This figure comes to roughly 3.1 pounds of mash consumed by each duck, which at time of shipment to point of release averaged approximately one pound in weight.

FATALITIES AND DISEASE

The principal known cause of fatalities in the 1944 rearing experiment was the paratyphoid organism, *Salmonella*, identified on three occasions in 1944 in dead ducklings sent to the Department of Animal Pathology, College of Agriculture, University of Illinois (now the College of Veterinary Medicine). Levine and Graham (1942) had previously reported this disease from young wood ducks that died during the first attempt at rearing this species by Survey personnel in 1941.

The species *S. typhimurium* was identified from liver cultures of several ducklings examined in 1941. This organism was believed to have been primarily responsible for the death of approximately 400 out of the 500 ducklings hatched in the 1941 rearing attempt; the genus *Salmonella* was assigned as the principal cause of death for 377 out of the 445 ducklings hatched in the second rearing attempt made in the spring of 1942.

A pasted vent was generally the most easily recognized symptom of *Salmonella* infection. In addition, infected ducklings became inactive and "mopey" and lost weight because of failure to eat, their normal oval body outline becoming compressed and flattened ventrally. Mortalities from the disease in 1944 were definitely increased in one pen through overcrowding. Most deaths occurred during the first five days after hatching; fatalities rapidly decreased with age, and no birds died from paratyphoid after attaining the age of three weeks.

Because paratyphoid organisms are presumably kept in circulation in the wild population by adult wood ducks that have recovered from the disease, there is always the possibility that some eggs collected in the wild may carry *Salmonella* organisms. Evidence obtained in the 1944 and earlier rearing projects indicated that the initial source of paratyphoid was probably from ducklings that had hatched from infected eggs. Evidence suggesting that some ducklings in the wild die from disease shortly after hatching, presumably having contracted the paratyphoid organism in the egg, was furnished by a brood of seven newly hatched ducklings which were found in a weak and sickly condition near Havana, Illinois. This entire brood died within a couple of days after being picked up and culture tests of one of these seven ducklings sent to the Animal Pathology laboratory at Urbana for study revealed that it was infected with *Salmonella*. Consequently, it would seem that the chief problem in connection with the propagation of wood ducks from eggs collected in the wild is controlling the spread of paratyphoid when it appears in the newly hatched young.

A few ducklings apparently died from eating some of the sawdust used as litter under the brooders. Several may have died because of failure to eat and at least two were cripples at hatching.

No other types of mortality were recorded.

DISCUSSION AND CONCLUSION

Unless some adequate explanation can be advanced as to why one experiment succeeded and other failed, values accruing from the successful experiment are necessarily limited. Reasons suggested here for the relative success of the 1944 wood duck rearing experiment must be largely circumstantial for the methods were not retested and evaluated in a second season.

The degree of hatching success (61 per cent) obtained in the 1944 experiment has little meaning because incubation was carried out under a regime prescribed for turkeys. Perhaps the chief reason for the relatively poor hatch was inadequate moisture conditions. At the Delta Duck Station, Delta, Manitoba, relative humidity is held near the saturation point and the eggs are sprayed with water several times daily. This latter procedure is also carried out by the Fin and Feather Club, located near Dundee, Illinois, in the incubation of mallard eggs. It is followed by commercial duck hatcheries when the eggs are near the hatching stage, according to Dr. H. M. Scott of the University of Illinois College of Agriculture.

Rearing success was high in 1944 primarily because losses from paratyphoid were held down. Important steps taken to insure good sanitation in the brooders and rearing pens, such as wire flooring and frequent changing of the water in the tanks, probably aided in controlling the spread of the disease. In the previous rearing experiments, the ducklings were kept indoors on sand-covered cement floors. Frequent use of water tanks by the ducklings kept the sand near the tanks continually soaked, creating ideal conditions for keeping *Salmonella* bacteria viable in droppings. Furthermore, wet sand has a tendency to cling to and mat the down of ducklings and in some cases probably causes chilling, thus reducing their vitality. At Havana, the pens were usually subjected to full direct sunlight which, according to Hinshaw (Biester and Schwarte 1948), is the best disinfectant known.

The abundance of insects secured at night under the lights, in addition to the fortified starting mash, apparently provided the ducklings with an adequate diet which is believed to have considerably enhanced their chances of resisting a paratyphoid infection.

LITERATURE CITED

BIESTER, H. E. and L. H. SCHWARTE. 1948. Diseases of Poultry. Iowa State College Press, Ames, Iowa. 1154 pp.

GREENBERG, DAVID B. 1949. Raising Game Birds in Captivity. D. Van Nostrand Co., Inc. N. Y. 224 pp.

LEVINE, N. D. and R. GRAHAM. 1942. Paratyphoid in Baby Wood Ducks. Jour. Am. Vet. Med. Assn. 100: 240–41.

MCCABE, ROBERT A. 1947. The Homing of Transplanted Wood Ducks. Wilson Bulletin 59: 104–109.

Accepted for Publication May 17, 1950.

A STUDY OF PRODUCTIVITY AND MORTALITY OF CORALLED DEER*

C. W. Severinghaus
New York State Conservation Dept., Albany, New York

Deer populations have been maintained under fence for many reasons. Europeans have held them for sporting and esthetic purposes, and for meat. Americans have done the same. In Texas a deer-production area has been established on a peninsula along the Gulf Coast in Aransas County (Wilke, 1947). In Michigan deer have been held in enclosures (Davenport, 1939; Davenport, et al., 1944) to determine the carrying capacity and other factors associated with various types of winter range. Deer also have been held on the George Reserve in Michigan where their productivity and other wildlife relationships have been studied (O'Roke and Hamerstrom, 1948). Deer were held in an enclosure in Ulster County, New York in 1896 for the purpose of restocking the Catskill forests (Whipple, 1935). For many years a population of approximately 100 deer was held in a 750-acre enclosure on the Burnham estate in Essex County, New York (Leopold, 1939). Lantz (1908, 1910) has described many other private and public deer parks over the nation but little has been published on the population changes in the enclosures he listed.

The original reason for establishing the deer enclosure used during this experiment was to provide information on the mortality rate of fawns between birth and their first winter. However, a great increase in population on the limited acreage of the enclosure during the first two years resulted in range destruction. Following this destruction, immediate steps were taken to reduce the herd and determine the physical condition of the remaining deer. Examination of the deer removed showed a sharp decline in reproductive rates as shown by breeding failures among adult does. In addition to information on fawn mortality, it became desirable, therefore, to determine the total picture of reproduction as it might relate to changes in population density and the altered browse conditions. It is the purpose of this paper to describe the annual population increments and losses and discuss their relationships with concomitant changes in the environment of the enclosure.

Materials and Methods

The deer enclosure used in this experiment is located at Partridge Run Game Management Area, township of Berne, Albany County, New York. The 86-acre area was fenced in 1941 with woven wire ten feet high. It enclosed a knoll which is at an elevation of 1,600 to 1,750 feet and is part of the southern New York section of the Appalachian Plateau. The soil is of low fertility, shallow, acid and a composite of stony silt loam overlying shale. The cover consists of small woodlots and farm-

* A contribution from Pittman-Robertson Wildlife Restoration Project 1-R and 28-R, New York State Conservation Department, Bureau of Fish and Wildlife Investigations.

lands abandoned about 1920 and consists of the following types by area:

	Types	*Acreage*
Open	—grass, etc.	24.35
Overgrown	—conifers, shrubs	20.30
Woods	—immature hardwood	15.85
	—immature mixed wood	11.50
	—mature mixed wood	5.75
	—mature conifers	5.42
Reforestation	—spruce, two to five feet high	2.50
Total		85.6

The most abundant browse species within reach of deer before the corral was stocked were hemlock (*Tsuga canadensis*), white pine (*Pinus strobus*), hard maple (*Acer saccharum*) and soft maple (*A. rubrum*). In addition there were 40 other species of trees and shrubs which contributed to the browse supply. Grasses, herbs and forbes were present but they did not contribute to the winter food supply because they were covered by snow. Therefore, they have not been listed.

The majority of the browse remaining in the corral after deer had been in it for four years were as follows: white pine, berry (*Rubus sp.*), wild cherry (*Prunus virginianus*) and alder (*Alnus sp.*). The hemlock was gone, very little hard or soft maple remained, and beech (*Fagus americana*) and hophornbeam (*Oystrya americana*) were completely browsed. A few heavily browsed specimens of the 40 other species of trees and shrubs remained along with grasses, herbs and forbes. Winter food for deer was practically eliminated from the corral.

Water was piped to a trough from a nearby pond and was available except for a few short periods when the pipes were frozen. Winter weather was rather severe and snow depths of two to four feet were not unusual during two to three months each winter.

The corral was free of deer until it was stocked. Deer used for stocking were from the southern part of the State. They were captured from the wild as fawns and raised in pens at the Wildlife Research Laboratory, Delmar, New York. In January 1942, 14 deer were placed in the corral—two adult males, five adult females and seven female fawns. In July 1943, seven fawns, four males and three females, eight weeks old, were added to the corral. In October 1943, four more deer were turned into the corral, one adult male, two adult females and one female fawn. These deer were all marked with numbered ear tags for later identification.

Eight of the deer stocked in the corral have, for the following reasons, been omitted from all calculations in this paper. The seven eight-week-old fawns stocked in July 1943 did not survive more than two months. There was also one female included in the stocking of January 1942 that was known to be a non-breeder when liberated. When she was removed in March 1946, an examination of her genitalia indicated chronic sterility. Although these deer added to competition for food, their failure to reproduce negates their further consideration respecting productivity.

Surveys were conducted each spring, summer, fall and winter to check on deer mortality in the corral. The entire area was fully combed by working parallel lines, north-south then east-west, with about 50-foot intervals between each member of the crew. In addition the area immediately surround-

ing the corral was searched. Whenever dead deer were found, their age (Severinghaus, 1949), sex and identification numbers, if ear-tagged, were determined and the remains removed from the corral. If parts of a dismembered deer carcass were found, all bones were measured, labelled, removed from the corral and saved for future reference. In this way all bones of a dismembered carcass were identified by matching skeletal members. Skeletal parts of some deer were found outside of the corral, but in every case such bones only added to skeletons already identified. This procedure of measuring and re-assembling all bones provided information as to whether miscellaneous bones belonged to a carcass already found or to some undiscovered carcass. Thus it was possible to determine exactly how many deer had been in the corral and eliminated the possibility of duplication.

When the study was terminated in March 1946 all deer remaining in the corral were either removed by live-trapping or shooting. In either case the age and sex of each deer was determined and each examined for ear-tags so that the year of birth could be ascertained. All live-trapped females were held in another set of pens until the summer following their removal to determine whether they were bred. These records and those taken during the experiment were used to determine the rate of reproduction as shown by survival at any period of the experiment.

FINDINGS

The record of stocking, productivity, survival and removal of deer during this experiment is shown in Table 1. A summary of the production and survival statistics is made in Table 2, and a summary of removal and losses is made in Table 3. The data given in these tables show that 13 deer vital to the experiment were stocked in January 1942. The four adult females among these produced six fawns the following June. During the fawning season of 1943, 17 fawns were produced by eight adult females.

These increases, the loss or removal of six deer during the winter of 1942–43 and the release of four more deer in October 1943, raised the population in the fall of 1943 to 34 deer vital to the experiment and the one deer excluded earlier because she was a non-breeder, or 35 deer on 85.67 acres (one deer to 2.45 acres). This density was more than the winter forage could sustain. The green skirts on hemlock, virtually down to the ground when the corral was stocked, were stripped of all needles to a height of seven feet. Sumac was eaten down to a heavy stem about a half-inch in diameter. All shrubs and hardwoods were heavily to completely browsed; berry canes were heavily browsed; and all annual growth was removed from hard and soft maples. As a result of the inadequate food supply, 16 deer died of malnutrition (Tables 1 and 3) before mid-March 1944.

With the range in this depleted condition, the remaining deer plus the spring fawn crop could not be carried through the following winter. Therefore, more deer were removed. Three of these were shot during March 1944 to determine their physical condition. These deer were very thin and the bone marrow in their femurs was red and gelatinous. Five other deer (Table 3)

TABLE 1.—RECORD OF STOCKING, PRODUCTIVITY AND SURVIVAL AND REMOVAL OF DEER HELD IN AN 86-ACRE ENCLOSURE AT PARTRIDGE RUN GAME MANAGEMENT AREA—1942–1946

Composition of Population		1942	1943									1944							1945				1946
						Losses						Losses				Losses			Losses				Losses
		Initial* Stocking	Survival**			Winter	Winter Mortality	Survival		Additions	Survival	Winter	Winter Mortality			Survival		Summer	Survival	Winter	Survival			Winter
Age	Sex	January	May	June	Nov.	Removal		May	June	Oct.	Nov.	Removal				May	June	Removal	Nov.	Removal	May	June	Nov.	Removal
Adult	Male	(2)	2	2	2	1		1	1	(1)	6	1	3			2	2	2	1	1				
	Female	(4)	4	4	11	1	2	8	8	(2)	10	3	2			5	5	2	8	5	3***	2	2	2
Yearling	Male								4								1							
	Female			7													5							
Fawn	Male			4	4			4	4		4	1	2			1	1	1				2	2	2
	Female	(7)	7	1	1		1		8	(1)	9		4			5								
	Unknown			1	1		1		5		5		5											
Total		(13)	13	19	19	2	4	13	30	(4)	34	5	16			13	14	5	9	6	3	4	4	4

* As has been noted elsewhere the original stocking composed a total of 14 deer. This included five adult females, whereas only four are listed in this table. The reason for this is the fact that one of these does is believed never to have bred as she was not observed with fawn in 1943 and an examination of the ovaries following her removal in March 1946 showed no evidence of breeding (corpora lutea).

** June figures for fawns represent those dropped the same spring. At this same time the surviving fawns of the preceding year are listed as yearlings. Similarly such yearlings are listed as adults in the November column.

*** One of these females disappeared and was not accounted for after the spring of 1945.

( ) Parentheses indicate stocked deer.

TABLE 2.—SUMMARY OF PRODUCTION AND SURVIVAL STATISTICS FOR DEER HELD IN ENCLOSURE AT PARTRIDGE RUN GAME MANAGEMENT AREA—1942–1946

Year	Winter mortality		Survival of breeders		Fawn production		Fall populations		Fall breeding sex ratio	
	Number	Per cent	Male	Female	Number of fawns	Number per female	Adults	Fawns	Male	Female
1941							6*	7*	1*	2.0*
1942			2	4	6	1.5	13	6	1	5.5
1943	4	21.1	1	8	17	2.1	16	18	1	1.7
1944	16	47.1	2	5	1	0.2	9		1	8.0
1945				2	2	1.0	2	2		

* As has been noted elsewhere the original stock was not liberated in the corral until January 1942. These figures, therefore, refer to these deer as they were held in the pens at the Delmar Research Center and are included so as to show the breeding ratio which existed in the fall of 1941 from which four fawns were produced in the corral in the spring of 1942.

TABLE 3.—RECORD OF REMOVAL AND LOSSES OF DEER HELD IN AN ENCLOUSRE AT PARTRIDGE RUN GAME MANAGEMENT AREA—1942–1946

			Reason for Removal or Loss						
Year	Age	Sex	Shot by poachers (late Nov. or Dec.)	Accidental death (in winter)	Winter mortality (malnutrition)	Determine physical condition (late winter)	Clearing enclosure of deer (summer or winter)	Lost but not recovered	Total
1942	Fawns	Male							
		Female							
		Unknown							
	Adults	Male							
		Female							
1943	Fawns	Male							
		Female		1					1
		Unknown		1					1
	Adults	Male	1						1
		Female	1	2					3
1944	Fawns	Male			2	1	1		4
		Female			4				4
		Unknown			5				5
	Adults	Male	1		3		2		6
		Female	1		2	2	2		7
1945	Fawns	Male							
		Female							
		Unknown							
	Adults	Male					1		1
		Female					5	1	6
1946	Fawns	Male					2		2
		Female							
		Unknown							
	Adults	Male							
		Female					2		2
Total			4	4	16	3	15	1	43

were removed in the summer of 1944 and were found to be still in poor flesh. Two other deer were killed by poachers in the fall of 1944 and left in the corral. This removal of 26 deer during 1944 (Table 3), which included the only fawn born that year, left nine deer in the corral (Table 1). These deer were sustained by the remaining foods, but overbrowsing continued.

The rate of fawn production during the period of adequate food supply was unusually high. During 1942 and 1943, 23 fawns were produced by 12 adult females, an average of 1.92 fawns each. Contrasting with this is the extremely low production for the two years following the heavy losses from malnutrition. In 1944 only one fawn was produced from among five adult females and in 1945 two fawns were produced from two adult females. This is an average of .429 fawns per adult doe for these latter years.

In addition to the above data on fawn production, further data were secured by examining deer removed during 1944 and 1945 for evidence of pregnancy. In 1944 two adult females were removed to determine physical condition (Table 3). One had a single female fetus and the other was not pregnant. Also, the two adult females removed in clearing the enclosure of deer (Table 3) were examined for pregnancy and neither had produced a fawn. In 1945 five adult females were examined upon removal from the enclosure, and one was found to be pregnant.

When these data on pregnancy are added to the data for 1944 and 1945 on fawn production, a total of 16 female deer of breeding age, yearling or older, produced five fawns or embryos. For the last two years of the experiment the combined data showed an average of .313 fawns or embryos per breeding doe.

This low productivity during the last two years raised the question of possible fawn mortality during the first six months of life. To discover this was the reason for initiating this experiment but, except for the fawns liberated at eight weeks of age in July 1943, no other fawns were found that had died previous to their first winter. Early fawn mortality was, therefore, negligible. It is believed that no young fawns, other than those noted, died during the experiment.

The loss of one female adult in the spring of 1945 (Tables 1 and 3) is the only unaccountable loss. What happened to her is still a mystery. Deer drives in the winter of 1945 showed, by adult counts, that she was present. Drives the following June showed she was missing. No other deer were known to have been lost without leaving some record by which the cause of loss could be determined.

DISCUSSION

It is believed that little error exists in the population data presented here and the fawn production calculated therefrom. With no deer to start with, no evidence of fawn mortality and knowing the age of all deer stocked and removed, the only source of error may have been from deer removed by poachers or animals which died before or shortly after birth leaving no trace of their demise. The mystery of one doe's disappearance has spoiled the record but the approximate date of her loss was known.

The experiment has produced data on gains and losses during a four-year period. The high rate of increase during the first two years (1.9 fawns per adult doe) is slightly higher than the rate of reproduction (1.7 embryos per adult doe) found by Morton and Cheatum (1946) from deer collected in the southern part of New York. The rate is also similar to that found by Hickie (1937) in deer on the George Reserve in Michigan. He reported an annual rate of increase of almost two fawns per adult doe for the first six years of observation in that enclosure. In all three areas high fawn production occurred during periods when a large variety of food was available from which deer could choose their diet.

Productivity in this experiment during the last two years was extremely low (.429 fawns per adult doe) and the reproductive rate (.313) attained by combining the data on pregnancy with that of fawns per adult doe was also low. During this period food was scarce and there was intense competition for that which was present. The productivity of the George Reserve herd showed that the early momentum (average increase of 60 per cent) contrasted sharply with the later history (O'Roke and Hamerstrom, 1948). Their work showed that during a six-year period following the years of high productivity the lowest productivity (36.7 per cent) was observed. Following herd reduction by shooting, productivity of the George Reserve deer came up to a point intermediate (53.8 per cent) to the extremes. It appears that the highest productivity came when there was the least competition for available food; that the lowest productivity came when there was the greatest competition for available food; and that the last period in the George Reserve was intermediate.

The work of Morton and Cheatum also showed that a low reproductive rate was probably associated with malnutrition. They found a reproductive rate of 1.2 embryos per adult doe in the northern region of New York where death from winter malnutrition is frequent (Severinghaus, 1947). Here again is another case where a lowered rate of reproduction is concomitant with great competition for the existing browse species, especially during the critical winter period.

High reproductive rates are not always due to the fawn production of adult does alone. It has been shown that in good deer range, 30 to 40 per cent of doe fawns may breed successfully (Morton and Cheatum, 1946). This condition was looked for in the present study. It must be pointed out, however, that the seven fawns placed in the corral on January 20, 1942 had not been with a sexually mature male previous to being stocked in the corral and, of course, none of them were observed with fawns during the summer of 1942. In June 1943 there were no females remaining in the corral that were born in 1942 (Table 1). In 1944 there were five females in the corral that were born in 1943 but none of these deer produced fawns. As a consequence, the possibility of bred fawns having been partly responsible for the high rate of increase during the first two years can be omitted from consideration.

The absence of losses among young fawns held in the corral also brings out a point which is often discussed.

In New York where deer predators are scarce, an explanation has been sought for the difference between a calculated fawn crop and the annual legal reported kill of deer. Administrators have often thought that a partial explanation for this difference was in young fawn losses. Yet such losses were not found in this experiment. Similarly, young fawn losses must have been very low in the George Reserve, otherwise the rate of increase found during the first six years could not have been attained. The observations from these two experimental populations tend to indicate that when food is abundant and man and predators are eliminated as decimating factors, the production of deer as shown by survival can closely approximate the theoretical maximum of the species.

SUMMARY

1. A high rate of increase (1.9 fawns per adult doe) was found during a two-year period in a deer population held in an enclosure when natural foods were abundant.
2. Following this two-year period, the area was severely overbrowsed and deer died during the winter from malnutrition.
3. A low rate of increase (.43 fawns per adult doe) was found during a two-year period after the enclosure was overbrowsed and when just enough natural food was present to sustain the population.
4. The study did not show any specific reason for the low rate of reproduction other than malnutrition resulting from a depleted food supply.
5. The study indicated that mortality among fawns between birth and their first winter was negligible.

LITERATURE CITED

DAVENPORT, LAVERNE A. 1939. Results of deer feeding experiments at Cusino, Michigan. Trans. 4th North Amer. Wildlife Conf., 268–274.

DAVENPORT, L. A., WARREN SHEPTON and W. CARL GOWER. 1944. A study of the carrying capacity of deer yards as determined by browse plots. Trans. 9th North Amer. Wildlife Conf., 144–149.

HICKIE, PAUL. 1937. Four deer produce 160 in six seasons. Mich. Cons. 7(3): 6, 7, 11.

LANTZ, D. E. 1908. Deer farming in the United States. U.S.D.A. Farmer's Bull. 330. Government Printing Office, Washington, D. C.

———. 1910. Raising deer and other large game animals in the United States. U.S.D.A. Biological Survey Bull. No. 36. Government Printing Office, Washington, D. C.

LEOPOLD, ALDO. 1933. Game Management. Charles Scribners Sons, New York, p. 481.

MORTON, GLENN H., and E. L. CHEATUM. 1946. Regional differences in breeding potential of white-tailed deer in New York. Journ. Wildlife Mgt. 10(2): 242–248.

O'ROKE, E. C., and F. N. HAMERSTROM, JR. 1948. Productivity and yield of the George Reserve deer herd. Journ. Wildlife Mgt. 12(1): 78–86.

SEVERINGHAUS, C. W. 1947. Relationship of weather to winter mortality and population levels among deer in the Adirondack region of New York. Trans. 12th North Amer. Wildlife Conf., 213–223.

———. 1949. Tooth development and wear as criteria of age in white-tailed deer. Journ. Wildlife Mgt. 13(2): 195–216.

WILKE, L. A. 1947. Deer on a production line. Outdoor Life 100(11): 40, 41, 86.

WHIPPLE, GURTH. 1935. Fifty years of conservation in New York State. J. B. Lyon Co., Albany, New York, pp. 199.

Accepted for Publication April 29, 1950.

CENSUSING ELK BY AIRPLANE IN THE BLUE MOUNTAINS OF WASHINGTON*

Helmut K. Buechner, Irven O. Buss, and Homer F. Bryan

Department of Zoology
State College of Washington, Pullman, Washington

The objectives of this study were: (1) To conduct an aerial census of elk (*Cervus canadensis nelsoni* Bailey) on their winter range in the Blue Mountains of southeastern Washington during the late winter and early spring of 1949 and 1950, (2) to test the accuracy of these aerial counts by comparing them with ground counts made on the same land and by comparing repeated aerial counts over the same area, and (3) to determine the practicability of an air-census technique in rugged terrain.

We extend grateful acknowledgment to Hobart G. Jenkins who piloted a Cessna 140 for fourteen hours during the study. Recognition is also given to Arthur E. Haines, Clifford A. Moser, and John M. Parrish, wildlife students, who acted as observers on three of the flights. Ground censuses were conducted by the State of Washington Department of Game with the cooperation of the U.S. Forest Service and staff and students of the State College of Washington.

Description of the Area

The Blue Mountains of Washington lie in the northern part of the Central Mountains subprovince of the Columbia Intermontane province (Freeman, Forrester, and Lupher, 1945). They include a complex group of mountain ranges and incised highlands that spread widely over central and northeastern Oregon and extend into southeastern Washington. Final deformation occurred in the early Pleistocene and was largely a regional uplift accompanied by local warping and faulting that marked out most of the present basins, plateaus, and ranges. The present surface is generally mature in the highest portions and youthful where canyons have cut into the uplifted young lava plains. In Washington the Blue Mountains are characterized by deeply dissected relief, steep, V-shaped canyons, knife-backed ridges, and conspicuous buttes. The highest elevation is about 6,500 feet at Oregon Butte.

Approaching the mountains from the north, one encounters a rapid, relatively uniform rise in elevation, the terrain becoming more abrupt and dissected in the mountains proper. Near the Washington-Oregon boundary in the Crooked Creek region the mountains are deeply eroded and rugged. The various parts of the mountains therefore offer different problems in air-census techniques.

Arid grasslands of Festuca-Agropyron and Agropyron-Poa (Daubenmire, 1942) characterize the area north of the Blue Mountains and Festuca-Agropyron dominates the south-facing slopes within the mountains. Although considerable blending occurs, zonation of communities is noticeable, passing from lower

* This investigation was supported in part by The State College of Washington Research Fund.

elevations through ponderosa pine, Douglas fir and grand fir, and finally to spruce-subalpine fir. Douglas fir dominates much of the timbered area, and grasslands occur on most of the south-facing slopes.

FLIGHT TECHNIQUES

Censusing elk by airplane is apparently successful in certain open areas of large relief, such as the Jackson Hole —Gros Ventre region (Olsen, 1944). The difficulties of counting elk in rugged topography and dense cover are mentioned by Riordan (1948, pp. 427, 429, and 430). Hunter (1945) reports the use of airplanes to determine population trends of deer and elk but does not give the details concerning techniques or the accuracy of counts made from the air. The practicality and accuracy of aerial counts of elk in rugged mountainous terrain apparently has not been adequately tested.

During February and early March when the elk were concentrated on the rolling, open terrain north and northeast of the mountains, a simple method of "strip cruising" was employed. Care was taken to keep the observer counting away from the sun. If the animals were concentrated in large compact groups, the plane was orbited until an accurate tally could be obtained. For relatively open country the best counting altitude appeared to be about 300 feet.

In moderately steep terrain best results were obtained by flying each drainage separately. Side canyons or draws were explored systematically by flying up one and returning to the main canyon by way of another. Where broad, steep canyons were encountered, it was necessary to fly up the drainage at canyon-top altitude to count elk on the upper half of the canyon wall and return down the canyon at almost tree-top level to record the lower elk. In rugged terrain contouring was resorted to most of the time; this proved to be the safest and most effective technique. Contouring was begun at the head drainages and progressed to lower elevations. Frequent returns were often necessary to cover the entire drainage accurately. Care was taken not to become "boxed in" by rapidly rising canyons. The chances of recounting or missing elk depended primarily on the skill of the pilot and observer in remembering landmarks.

It was particularly evident when counting in canyons that the observer had to be flown close to the elk to assure accurate enumeration. This necessitated low and skillful flying.

The Piper Cub had a distinct advantage over the Cessna 140 in its slower airspeed (60–70 mph compared with 70–100 mph) and was more maneuverable in short canyons. On the other hand, the more stable flight characteristics at high altitude and greater horsepower of the Cessna 140 enabled it to climb out of side canyons and draws where extra power assured a comfortable margin of safety.

RESULTS

Census figures are presented in Table 1. Where more than one count was made over an area, the largest figure was used. The large Mill Creek watershed east of Walla Walla was not surveyed by us in 1949, since personnel of the State of Washington Department of Game made both aerial and ground

TABLE 1.—COMPARISON OF AERIAL AND GROUND COUNTS

Location	Aerial Count		Ground Count					
	Number of Elk		Counted		Estimated		Total	
	1949	1950	1949	1950	1949	1950	1949	1950
Mill Creek	821	87**	1003	—	60	—	1063*	
Tucannon River	82	45**	241	287	57	86	298	373
Abel Ridge	255	289**	371	346	55	35	426	381
Pomeroy-Alpowa Creek	117**	0	—	—	—	—	—	—
Grande Ronde River	70	—	59	—	50	—	109	—
Asotin Drainage to Grande Ronde	494	296***	787	565	259	104	1046	669
Mount Misery	8**	—	—	—	—	—	—	—
Touchet Drainages	49	94**	27	41	96	28	123	69
Wenaha-Crooked Creek	436**	—	—	—	—	—	—	—
Total	2332	811	2488	1239	477	253	3065	1492

*325 estimated as elk from Oregon wintering in Washington.
**None or only small part of area covered by ground crews.
***Pinkham Butte area only; 209 not counted by ground crews.

counts that compared favorably (821 by airplane; 1,003 by ground crews).

On the area where aerial and ground counts were duplicated in 1949, the airplane count was 950 elk and the ground count was 1,485. The greatest discrepancies occurred in the Tucannon River area and the extensive area designated as Asotin Drainage to Grande Ronde. In the Tucannon area only 82 elk were counted from the airplane while 241 were counted by ground crews. This area has considerable timber on north-facing slopes and in the bottoms of numerous canyons. Elk were seen in the timber when the airplane passed almost directly over the animals, but many obviously were missed since the area viewed vertically was relatively small compared with the entire area censused. In the Asotin drainage system only 494 elk were seen from the airplane in contrast to 787 elk seen by the men on the ground. These counts were made on March 5 and February 1 and 2, respectively. A greater discrepancy was noted when the aerial count was made over this area on February 5, when only 177 elk were seen. Windy weather during this count affected flying and the behavior of the animals. The elk showed definite tendencies toward seeking shelter in timbered canyons during cold, windy weather and this reduced the aerial count. It should be noted that the Asotin area is extensive, covers about 150 square miles, consists largely of extensive grasslands, and areas in timber where elk are likely to be overlooked are considerably restricted. Yet, relatively few elk missed per square mile may accumulate a sizeable error.

Hazardous flying weather during nearly all of the late winter and early spring of 1950 together with limitation of flying to weekends precluded extensive aerial counts to cover all of the area surveyed the previous year. Integration of aerial and ground censuses was greatly improved in 1950, with the result that 574 elk were counted from the air in areas inaccessible to men on the ground. Unfortunately, no counts were made in the important Wenaha-Crooked Creek area and flying condi-

tions were somewhat unfavorable during the count in the Mill Creek area where only 87 elk were seen.

TABLE 2.—COMPARISON OF REPEATED COUNTS

	Number of Elk Seen	
Date	Wenaha-Crooked Creek	Asotin Drainage
2– 5–49	—	177
2–24–49	0	—
3– 5–49	56	494
4– 2–49	436	—

Table 2 shows considerable discrepancy among repeated counts made in two areas. These counts are open to criticism; obviously the logical procedure is to use the largest figure. There is a great need for series of repeated counts over given areas to determine variability and measure the reliability of aerial counts. When flying over an area the observer actually sees only a portion and not the total number of elk present. The variability of this portion cannot be determined by only one count. Repeated counts are necessary to build up a series of counts that may be analyzed to determine the amount of error. Obviously the maximum count in a series represents the closest approximation of the herd. This figure may not be attained with one flight alone. The usual aerial census envolves only one flight over a green area. So far, few reliability tests have been made to determine the accuracy of such single flights. Each area possesses its own complex set of factors that cause variation in aerial counts; therefore, reliability standards are needed for each management unit. The different areas reported here for the Blue Mountains vary sufficiently in topography and vegetation to require separate statistical treatment. Little confidence may be placed in a single aerial count when the population is unknown and no data exist to indicate the probable magnitude of error. In the past aerial counts were accepted because the observers "felt they had a good count" or because the count compared favorably with a ground census. The former is unscientific and the latter is subject to the errors of ground censusing. An apparent need exists for repeated aerial counts on winter range under ideal conditions of weather, time of day, and time of year. In this manner the variability of airplane censusing may be determined.

Several of the western states use airplanes in censusing elk, but no known detailed studies have been conducted to determine the reliability of such counts in rough, mountainous terrain.

SOURCES OF ERROR

Errors in the present aerial census were numerous. Weather appeared to influence elk behavior considerably. Local movements into shelter of timbered draws and canyons occurred in the Abel Ridge and Asotin areas on cold, windy days. Major seasonal movements occurred in the Wenaha-Crooked Creek area with the advent of spring weather. Censusing in this part of the mountains was best early in April after the elk had started their migration toward summer range. Calm, cloudless days were chosen for flights when possible. Early March appeared to be the best time for airplane counting over the area in general in 1949. Approximately two weeks of good weather occurred at this time, beginning February 24. The elk were still on winter range, feeding on open ridges and

grassy, south-facing slopes. This time appeared ideal for counting the Asotin drainage and the Abel Ridge area. Unfortunately, the weather was mostly unsuitable for flying during the remaining portion of March. On winter range in the vicinity of Troy, Oregon, elk were difficult to count in the heavy timber of canyon bottoms. By April 2 the elk were moving up the drainages along the grasslands of south-facing slopes where they could be counted more easily. A reconnaissance of the same area in May showed that the elk were widely dispersed on summer range in small canyons and draws. Most of the elk were seen as individuals, and nine was the largest number seen in one herd. At this time, many elk were found beneath the canopy of coniferous timber, making censusing impractical. Only 102 elk were seen on May 5 over the same area where 436 were observed April 2.

Limitations of airplane flying in the rugged terrain were responsible for an appreciable error, particularly in the Crooked Creek country. Many small canyons and draws could not be inspected thoroughly even when attempted by hazardous flying. The number of elk overlooked in such places was of unknown magnitude and may have been considerable.

Early morning light also influenced the accuracy of airplane censusing. Whereas it was desirable to census as soon as possible after daybreak when the elk were most active, whole hillsides were virtually blotted out when the observer looked toward the sun. These slopes appeared misty and hazy, and many elk were probably missed under such conditions. The difficulty applied only in deep canyons and was overcome in part by counting an hour or two later in the day.

ESTIMATED ELK POPULATION

The actual number of elk seen by ground crews in 1949 was 2,488. Of this number 325 were estimated to be Oregon elk wintering in the Mill Creek drainage, which leaves 2,163 elk for Washington. An additional 561 were seen from the airplane, making a total of 2,724, as shown in Table 3. Without doubt some elk were inadvertently missed during the counting. Since the magnitude of this error is unknown, it would be inadvisable to make large additional estimates to the population counted.

The data presented in Table 3 show an increase from 1,610 to 1,726 elk in the area exclusive of the Mill Creek and Wenaha-Crooked Creek areas. In this area plus the large Crooked Creek drainage, 1,309 elk were killed during the 1949 hunting season and 102 were killed during the special season which extended to March 1, 1950 on the

TABLE 3.—COMPARISON OF ELK ACTUALLY SEEN, 1949 AND 1950

	Exclusive of Mill Creek and Wenaha-Crooked Creek					
Year	Air-Count Supplement	Ground Count	Subtotal	Wenaha-Crooked Creek	Mill Creek	Total
1949	125	1,485	1,610	436	678	2,724
1950	487	1,239	1,726	—	87	1,813

northern edge of the mountains. The kill figure, 1,309, also represents elk from the Mill Creek drainage system harvested while they occupied summer range above the watershed. These figures alone do not indicate the population trend for the mountains as a whole. On the Mill Creek watershed 502 elk were reported killed during a special postseason hunt (November 15 to March 1). An unknown number was taken legally in this area but not reported, and an unknown number was taken illegally. The magnitude of the unknown figures may have been considerable, as indicated by the count of only 87 elk in the Mill Creek drainage on February 26. Apparently a marked herd reduction occurred in the Mill Creek area.

An examination of Table 4 shows that the total kill from 1943 to 1948 averaged 737 elk and that the reported kill of 1,913 in 1950 was more than twice the kill made in any one previous year, except 1946. By applying Kelker's formula (Kelker, 1947) to the last ten years in Leopold's table (Leopold, 1933, p. 456) for the breeding potential of animals bearing their young at three years of age and having only one young, the theoretical maximum increase for elk was determined as 26 per cent. If it is assumed that the population was more or less stable during the period from 1943 to 1948 at about 3,000 elk (as indicated by the 1949 census), the average harvest of 737 would represent a rate of increase of about 24 per cent. Indubitably some elk were lost through crippling and other decimating factors. The actual rate of increase therefore lay between 24 and 26 per cent. Obviously reproduction was exceptionally high and near the theoretical maximum. From these figures it seems likely that about 750 breeding cows were represented in the population and that a sustained annual harvest of about 700 elk was possible. To maintain the population, this harvest should be equally divided among males and females. It was also evident that the total harvest during the fall and winter of 1949–1950 was considerably in excess of the reproductive gain for 1949, most of the reduction occurring in the Mill Creek area. This reduction was made purposely to appease landowners who had complained of damage to haystacks and wheatfields.

The above interpretation is based solely on actual data. However, if the

TABLE 4.—ELK KILL STATISTICS, 1943–1949

Year	Bulls	Cows	Calves	Not Classified	Total
1943	99	317	152	123	691
1944	256	185	109	135	685
1945	459	274	89	—	822
1946	502	342	148	—	992
1947	342	87	52	69	550
1948	410	197	74	—	681
1949	594	923	396	—	1913
1949*	(170)	(184)	(148)	—	(502)
Average 1943–1948					737

*Special Mill Creek season.

census figures are low, several departures could occur: (1) The rate of increase could be somewhat less than 24 per cent, (2) losses caused by decimating factors other than hunting could be higher than indicated, and (3) the sustained harvest might be somewhat above 700.

SUMMARY AND CONCLUSIONS

Elk were counted by airplane in the Blue Mountains from February 1 to April 4, 1949 and during February, 1950. Part of this work was carried out concurrently with ground censuses made February 1 and 2, 1949, and February 12, 1950. A total of 1,511 elk was counted from the air in 1949. On the area covered by both air and ground counts, 950 elk were counted from the airplane and 1,485 from the ground. If the airplane had not been employed, 561 elk would have remained uncounted in 1949 and 574 in 1950, indicating the value of combining air and ground techniques. The total seen during both air and ground observations was 2,724 in 1949 and 1,813 in 1950. Because of adverse flying weather the census for 1950 was incomplete for the Wenaha-Crooked Creek, Grande Ronde, and Mount Misery areas, and possibly inaccurate for the Mill Creek drainage. Without doubt some elk were inadvertently missed during these counts by direct observation. Little confidence may be placed in single-flight counts over areas characterized by deep, V-shaped canyons; heavily dissected topography with many canyons and draws that prohibit complete coverage; and coniferous-covered, north-facing slopes and draws. Repeated counts, both aerial and ground, are necessary to determine variability of census data and limits of accuracy. It seems reasonable to employ a figure of about 3,000 elk for the posthunting-season population in 1949. While the true population level may have been above 3,000, a higher figure could not be supported on available data. The annual increment appears to be between 24 and 26 per cent, and the sustained productivity harvestable by hunters seems to be about 700 elk. Both sexes should be harvested in approximately the same numbers if the population is to be maintained at a relatively stable level.

LITERATURE CITED

DAUBENMIRE, REXFORD F. 1942. An ecological study of the vegetation of southeastern Washington and adjacent Idaho. Ecol. Mono. **12**: 53–79.

FREEMAN, OTIS W., J. D. FORRESTER, and R. L. LUPHER. 1945. Physiographic divisions of the Columbia Intermontane Province. Annals Assoc. Amer. Geographers **35**: 53–75.

HUNTER, GILBERT N. 1945. Methods of determining trends in big game numbers and range conditions. Trans. N. Amer. Wildlife Conf. **10**: 234–241.

KELKER, GEORGE HILLS. 1947. Computing the rate of increase for deer. Jour. Wildlife Management **11**: 177–183.

LEOPOLD, ALDO. 1933. Game management. Charles Scribner's Sons, New York. xxi + 481, see p. 456.

OLSEN, ORANGE A. 1944. Elk below! Amer. Forests **50**(2): 65–67, 94.

RIORDAN, LAURENCE E. 1948. The censusing of deer and elk by airplane in Colorado. Trans. N. Amer. Wildlife Conf. **13**: 409–430.

Accepted for publication July 11, 1950

A STUDY OF POPULATIONS OF FISH BASED ON CATCH-EFFORT STATISTICS

D. N. Omand
Ontario Dept. of Lands and Forests, Toronto

The Department of Lands and Forests of the Province of Ontario conducts an annual program of netting small-mouth black bass (*Micropterus dolomieu*) from waters in the Province where these fish are abundant, but small. The fish are transported to waters where angling is reported to be poor. In the course of this work many non-game fish are taken in the nets. These are destroyed, since it is believed to be desirable to exert the same fishing pressure on all species present in order that the relative population levels in the waters will not be seriously influenced. The present paper is an attempt to use the catch-effort data of three such fisheries to assess the populations of the lakes. O'Reilly Lake, Davern Lake and Little Silver Lake, in Eastern Ontario, north of the Kingston-Gananoque area are dealt with here.

In accordance with our usual method, trap nets were set in each lake, and catches recorded from day to day. It was found that a net set in a likely looking spot would fish well for a few days, when catches would drop off sharply. The net was then moved, in some cases only about 200 yards, and high catches would again be reported. The entire impression was one of exploitation of a series of local populations, usually in sheltered bays. The setting of the trap net is limited to rather shallow water (up to ten feet) and no effort at exploitation of those areas where shore water was over this depth was made.

Fishing was carried on with six-foot trap nets with 100 ft. leads of 2½-inch mesh and 2-inch mesh cribs. These nets are of the usual commercial type, made up for work of this nature. Nets of the same size were selected for purposes of standardization. They were lifted every day and moved when fishing began to drop off. Such nets retained no fish of less than six inches fork length, and the following discussion therefore refers only to fish of greater size than this. For the purpose of calculation, one unit of effort is taken as one trap net set for twenty-four hours, and the unit of catch is one fish. The time unit is one day.

Figures for daily catches given below are total figures for all nets on the day concerned. The catch of an individual net would drop off sharply from day to day in any one location, as explained above. Frequent moving of those nets which were fishing poorly served to keep the catches at a fairly good level throughout the experiment, and at the same time spread the fishing effort over a considerable portion of the lake. These figures, therefore, apply to the daily fishery conducted over the majority of the lake rather than to local areas.

The method of population estimation used here was described by De Lury (1947). It is based upon the belief that the number of fish taken from a popu-

lation by successive units of effort is governed by the total population available and hence the population may be estimated on the basis of the observed catch per unit of effort.

De Lury bases his calculations on three assumptions.

(1) The population is closed. The effects of migration and natural mortality are negligible.

An endeavour was made, by fishing intensively for the shortest possible period, to satisfy this requirement. Undoubtedly, a certain number of individuals entered the size group dealt with during the period of netting, and some natural mortality and angling mortality occurred, but it is believed that this is negligible in comparison with the total number of fish removed.

(2) That the units of effort do not compete with one another.

It is believed that the nets were set in such a way that no two were competing during a given time interval.

(3) That catchability, defined as the proportion of the total population remaining in the lake taken by one unit of effort, is constant.

While it is expected that day to day variations such as a decreased catch after a cool night, will occur, this assumption implies the absence of trend in catchability during the sampling period.

In the following tables 1, 2 and 3, column 1 refers to the time interval concerned and column 2 to the number of units of effort exerted in that time. Column 3 refers to the cumulative fishing effort up to time (t), (i.e. at t–1). The symbol c(t) refers to the catch in the time interval concerned and C(t) to the catch per unit effort per unit time or c(t)/e(t); K(t) is the cumulative catch up to time (t). These last three columns are given for each species of fish taken in numbers in the lake con-

TABLE 1.—CATCH STATISTICS FROM DAVERN LAKE

			BASS			SUNFISH			PERCH			ROCK BASS		
t	e(t)	E(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)
1	7	0	71	10.1	0	205	29.3	0	85	12.1	0	21	3.0	0
2	7	7	96	12.2	71	240	34.2	205	210	30.0	85	43	6.2	21
3	7	14	57	8.1	167	425	60.7	445	65	9.3	295	42	6.0	64
4	7	21	21	3.0	224	680	97.2	870	282	40.3	360	107	15.3	106
5	7	28	23	3.2	245	240	34.3	1550	76	10.8	642	71	10.2	213
6	7	35	34	4.8	268	195	27.9	1790	70	10.0	718	57	8.2	284
7	7	42	10	1.4	302	405	57.8	1985	112	16.0	788	82	11.7	341
8	7	49	24	3.4	312	336	48.0	2390	54	7.7	900	66	9.4	423
9	7	56	20	2.8	336	330	47.2	2726	70	10.0	954	51	9.3	489
10	7	63	7	1	356	167	23.9	3056	54	77	1024	35	5.0	540
11	7	70	8	1.1	363	248	25.4	3223	50	72	1078	10	1.0	575
		77			371			3741			1128			585

t — Time
e(t) — Effort per unit time
E(t) — Cumulative Effort.
c(t) — Catch per unit time
$C(t)=\frac{c(t)}{e(t)}$ —
K(t) — Cumulative catch.

TABLE 2.—CATCH STATISTICS FROM LITTLE SILVER LAKE

			BASS			BULLHEADS			PERCH			SUCKERS			SUNFISH		
t	e(t)	E(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)
1	7	0	131	18.7	0	101	14.4	0	85	12.1	0	27	3.9	0	31	4.4	0
2	7	7	69	9.7	131	154	22.0	101	40	5.7	85	44	6.3	27	57	8.1	31
3	7	14	99	14.1	200	128	18.3	255	41	5.9	125	62	8.9	71	48	6.9	88
4	7	21	78	11.1	299	104	14.9	383	47	6.7	166	52	7.4	133	80	11.4	136
5	7	28	56	8.0	377	75	10.7	487	49	7.0	213	23	3.3	185	74	10.6	216
6	7	35	76	10.9	433	83	11.9	562	62	8.9	262	30	4.3	208	69	9.9	290
7	7	42	49	7.0	509	67	9.6	645	52	7.3	324	36	5.1	238	83	11.9	359
8	7	49	42	6.0	558	56	8.0	712	68	9.7	376	23	3.3	274	51	7.3	442
9	7	56	63	9.0	600	87	12.4	768	76	10.9	444	24	3.4	297	74	10.6	493
10	7	63	47	6.7	663	61	8.7	855	59	8.4	520	18	2.6	321	68	9.7	567
		70			710			916			579			339			635

t — time
e(t) — effort per unit time
E(t) — cumulative effort
c(t) — catch per unit time
$C(t)=\frac{c(t)}{e(t)}$ —
K(t) — cumulative catch

TABLE 3.—CATCH STATISTICS FROM O'REILLY LAKE

			BASS			SUNFISH			BULLHEADS		
t	e(t)	E(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)	c(t)	$C(t)=\frac{c(t)}{e(t)}$	K(t)
1	4	0	167	41.75	0	42	10.0	0	57	16.25	0
2	4	4	86	21.5	167	36	9.0	42	40	10.0	57
3	4	8	98	24.5	253	56	14.0	78	32	8.0	97
4	4	12	102	25.5	351	21	5.25	134	17	4.25	129
5	4	16	95	23.75	452	19	4.75	155	28	7.0	146
6	4	20	114	28.5	548	26	6.50	174	21	5.25	174
7	4	24	98	24.5	662	24	6.0	200	20	5.0	195
8	4	28	88	22.0	760	17	4.25	224	14	3.5	215
9	6	32	133	22.2	848	41	6.9	241	27	4.5	229
10	6	38	137	22.8	981	38	6.3	282	31	5.0	256
11	6	44	101	16.8	1118	37	6.1	320	14	2.3	287
12	6	50	99	16.5	1219	31	5.1	357	19	3.2	301
13	6	56	101	16.8	1318	28	4.7	388	17	2.8	320
14	6	62	98	16.4	1419	29	4.9	416	8	1.3	337
15	6	68	101	16.8	1517	42	7.0	445	14	2.3	345
16	6	74	97	16.2	1618	38	6.6	487	10	1.7	359
17	6	80	94	15.7	1715	36	6.0	525	12	2.0	369
18	6	86	84	14.0	1809	42	7.0	561	4	.8	381
					1894			603			381

t — Time
e(t) — Effort per unit time
E(t) — Cumulative effort
c(t) — Catch per unit time
$C(t)=\frac{c(t)}{e(t)}$ —
K(t) — Cumulative catch

cerned. These symbols are taken from De Lury (1947).

If the assumptions indicated above are valid, De Lury has shown that $C(t) = k\,N(o) - k\,K(t)$ where k is the proportion of the remaining population captured by one unit of effort, which is constant by assumption (3) above, and N(o) is the number of fish present at time o or at the beginning of the experiment. If therefore, C(t) is plotted against K(t), a straight line relationship should appear, and the intercept of this line on the K(t) axis should be an indication of N(o), or the total population from which the sample is taken, since this will be the limit of cumulative catch when $C(t) = 0$.

It is therefore assumed that if a straight line relationship is, in fact,

observed when these plots are made, then the fishing effort in question was so applied that substantial inroads were made upon the numbers of the species concerned. In addition, where a straight line relationship is admissible, it is logical to believe that if the points through which the line is drawn converge upon the line itself in the latter stages of the sampling, the above assumption is supported by the data, since, if the fishing effort is reducing the population materially, the limits within which variance can occur will be reduced as the population is reduced by successive units of effort. In other words, if, when daily catch per unit effort is plotted against cumulative catch, a pattern of points appears through which a straight line of negative slope may be drawn, and if the points converge upon the line in the latter stages, then it is assumed that the fishing effort concerned is being exerted efficiently on the population under consideration. If, on the other hand, no such pattern appears, then it is assumed that the main body of the population under consideration is not in the area in which fishing pressure is being exerted, and those individuals which are being taken represent stragglers moving at random on the fringes of the main body of the population, or that the entire population is moving continuously, movement into the fishing area compensating or exceeding removal by the fishing effort. This is in keeping with De Lury's original calculations, since fishing effort is exerted only in a localized area, and the straight line relationship is assumed only if migration into the area where fishing effort is exerted is negligible. This condition would not obtain where the species is represented in the catch only by stragglers or by a continually moving population since migration into and out of the fishing area would be quite haphazard.

Data are presented for three lakes. In each case, the nets were set primarily for the taking of bass and the catching of other species was incidental to this. Records were kept of day to day catches of each species.

Davern Lake

This is a small lake lying in South Sherbrooke township of Lanark county. It lies in a heavily wooded valley, and generally the drop off from the shore is quite steep. For this reason, only about half of the shoreline was fishable by trap nets. The area is about 100 acres and angling is limited to a few boats a week, the lake being relatively inaccessible. Fishing in this lake was conducted from August 30 to September 9, 1949, and during that time a total of 371 smallmouth black bass (*Micropterus dolomieu*), 3471 sunfish (*Lepomis gibbosus*), 1128 perch (*Perca flavescens*) and 585 rock bass (*Ambloplites rupestris*) were removed. Table 1 shows the details of the daily catches in this lake. Symbols are taken from De Lury (1947). In Fig. 1, daily catch per unit effort C(t) is plotted against cumulative catch K(t). An examination of this figure (1A) shows that although over 3000 sunfish were removed from the lake, there is no indication of the points coalescing into the typical pattern described above. The points for both perch and smallmouth bass show the pattern typical of an effective fishing effort, the straight line relationship holding and the points

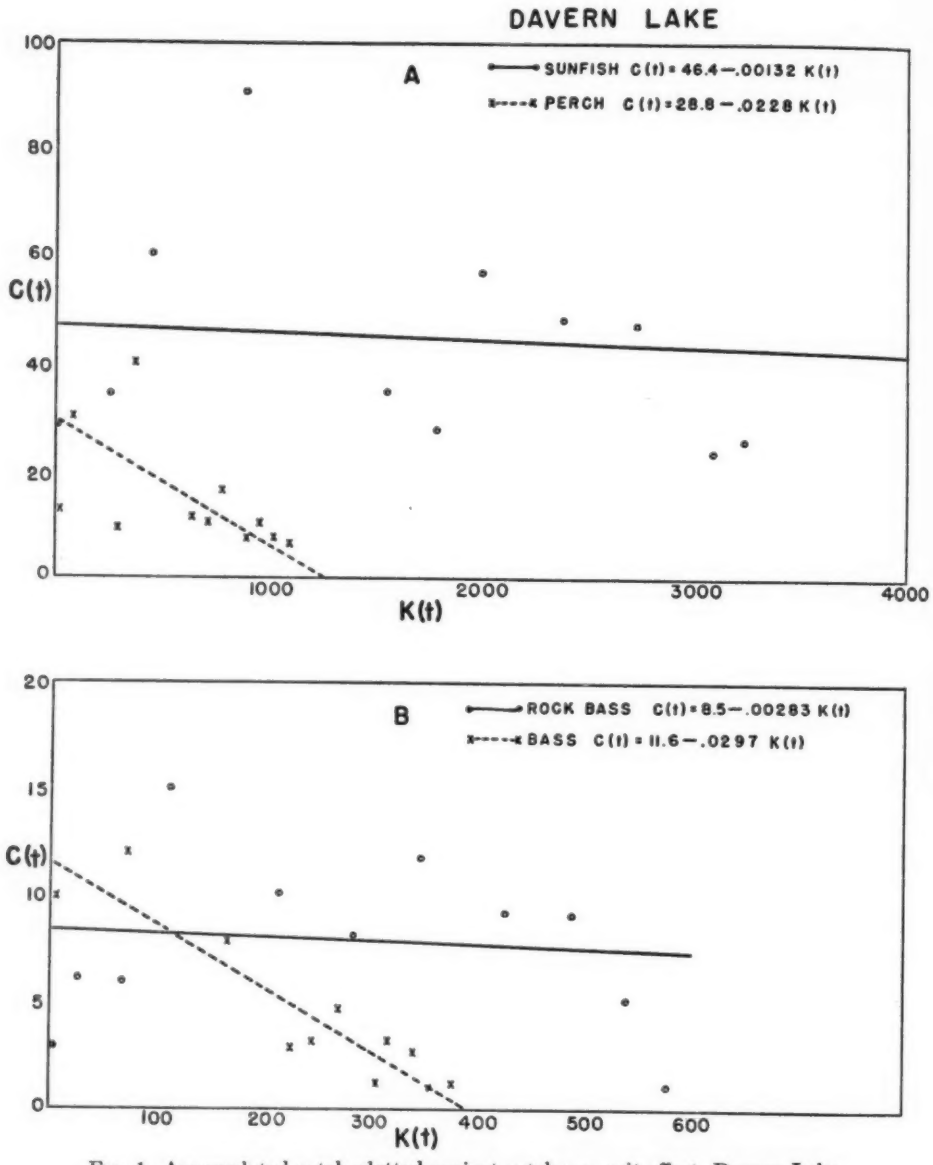

FIG. 1. Accumulated catch plotted against catch per unit effort, Davern Lake.

converging on the line as K(t) becomes larger (Figs. 1A and B). In the case of the rock bass it appears that the fishing effort exerted had little effect in reducing the total population materially.

LITTLE SILVER LAKE

This is a small body of water lying in South Sherbrooke township of Lanark county, about two miles north of Davern Lake. It is about the same size

as Davern Lake (100–125 acres), but is long and narrow in shape. It is more heavily fished than Davern Lake. The greatest difference is in the character of the shore water, which is shallower and much more susceptible to trap net fishing. About 80% of the shoreline was fished by trap nets from the period September 13 to September 22, 1949. During this period 710 smallmouth black bass, 916 bullhead catfish (*Amieurus nebulosus*) 579 perch, 339 suckers (*Castostomus commersonnii*) and 635 sunfish were taken. The details of the catch are shown in Table 2 and Figure 2 shows the plot of C(t) against K(t) for each species. An examination of fig. 2A shows that the plot for bullhead catfish conforms very well to the configuration believed to be typical of an effective fishing effort. This is true also of the plots of data on suckers (Fig. 2B) and on bass (Fig. 2C). The plot for perch (Fig. 2B) however shows no indication that the population is being reduced by the fishing effort. In fact, the line has a positive slope, indicating that recruitment to the population within the fishing area is taking place faster than removal by fishing effort. The same applies to the plot for sunfish, (Fig. 2C). A similar tendency was noted in Davern Lake.

O'Reilly Lake

O'Reilly Lake lies in Olden township of Frontenac county, and is about 100 acres in area. Angling is light, and the shoreline was such that about 70% of the lake was fishable by trap nets. Fishing was carried on for 18 days beginning August 2, 1949, and in that time 1894 smallmouth bass, 603 sunfish and 381 bullheads were taken. Details of the fishing are set out in Table 3 and Figure 3. Examination of this figure reveals once again that points for both bullheads and smallmouth bass lie along an admissable straight line, and show the convergence already described, while the points for sunfish again show no definite relationship.

Discussion

The information given above is summarized in Table 4. In each case the equation for the line of best fit as determined by the method of least squares is given, together with N(o) the estimate of total population as given by the intercept of the line of best fit on the K(t) axis. A qualitative estimate of the conformity to the pattern believed to be produced by efficient fishing effort is also indicated.

An examination of Figs. 1, 2, and 3 and Table 4 indicates that in each of the three lakes, the plot for bass conforms to the pattern predicted on the basis of previous assumptions. Since the fishing effort was directed to the capture of this species, and net sites were chosen with this in mind, such a conformity was to be expected. It is noteworthy that a very small estimate of total population is derived from Davern Lake and the graphic plot shows that the bass population reached a low level on completion of fishing. This may be related to the fact that less than half of the total shoreline was dealt with in fishing. Undoubtedly, if the entire shoreline had been fishable, the total numbers taken would have been greater, but the low level of fishing attained indicated that there was much less migration from unfished to fished areas

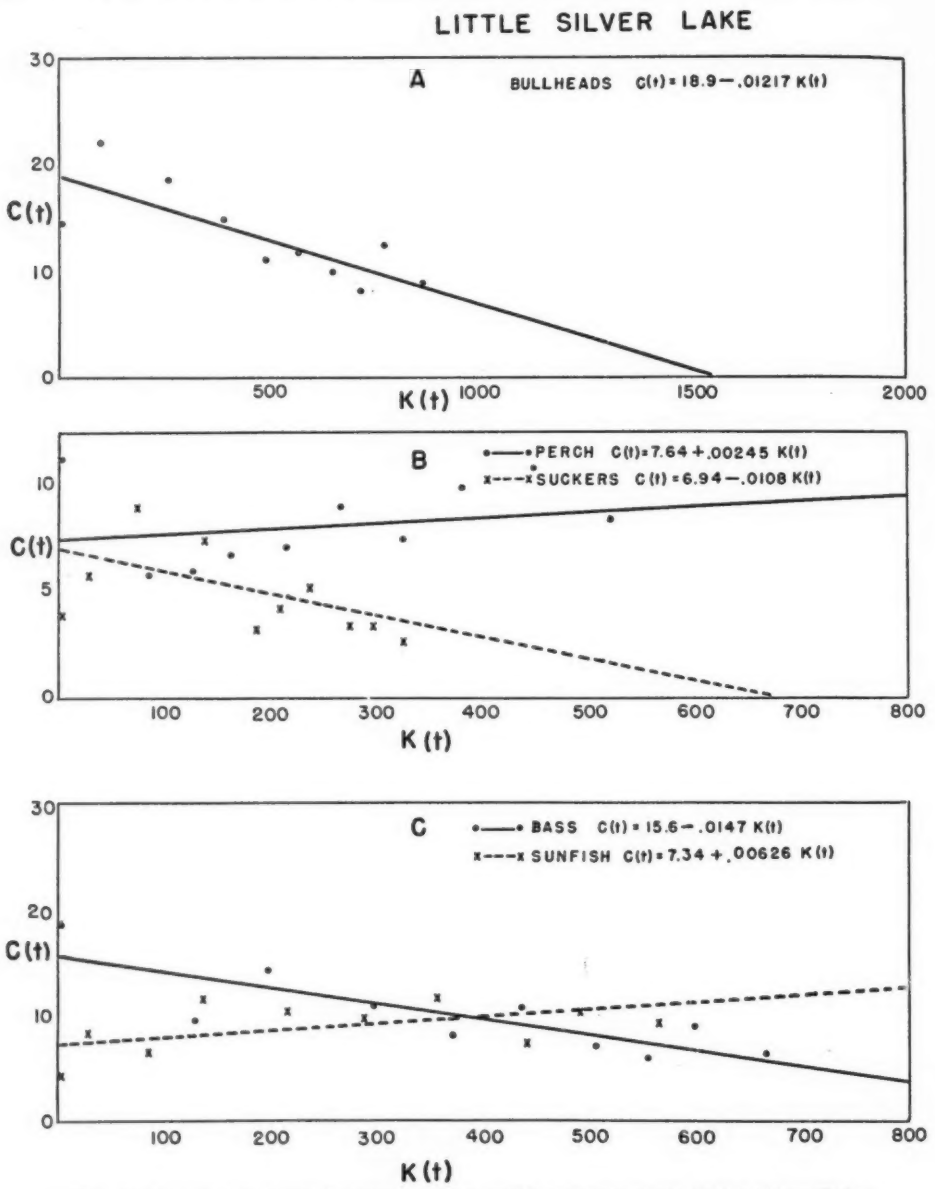

FIG. 2. Accumulated catch plotted against catch per unit effort, Little Silver Lake.

than might be expected, on the belief that drastic reduction of a population in a limited area is followed by migration into that area from outside. This may be related to the conditions under which the fish were living in the lake, where possibly there was a good balance between population and environment, with few individuals occupying marginal habitats. It may also be

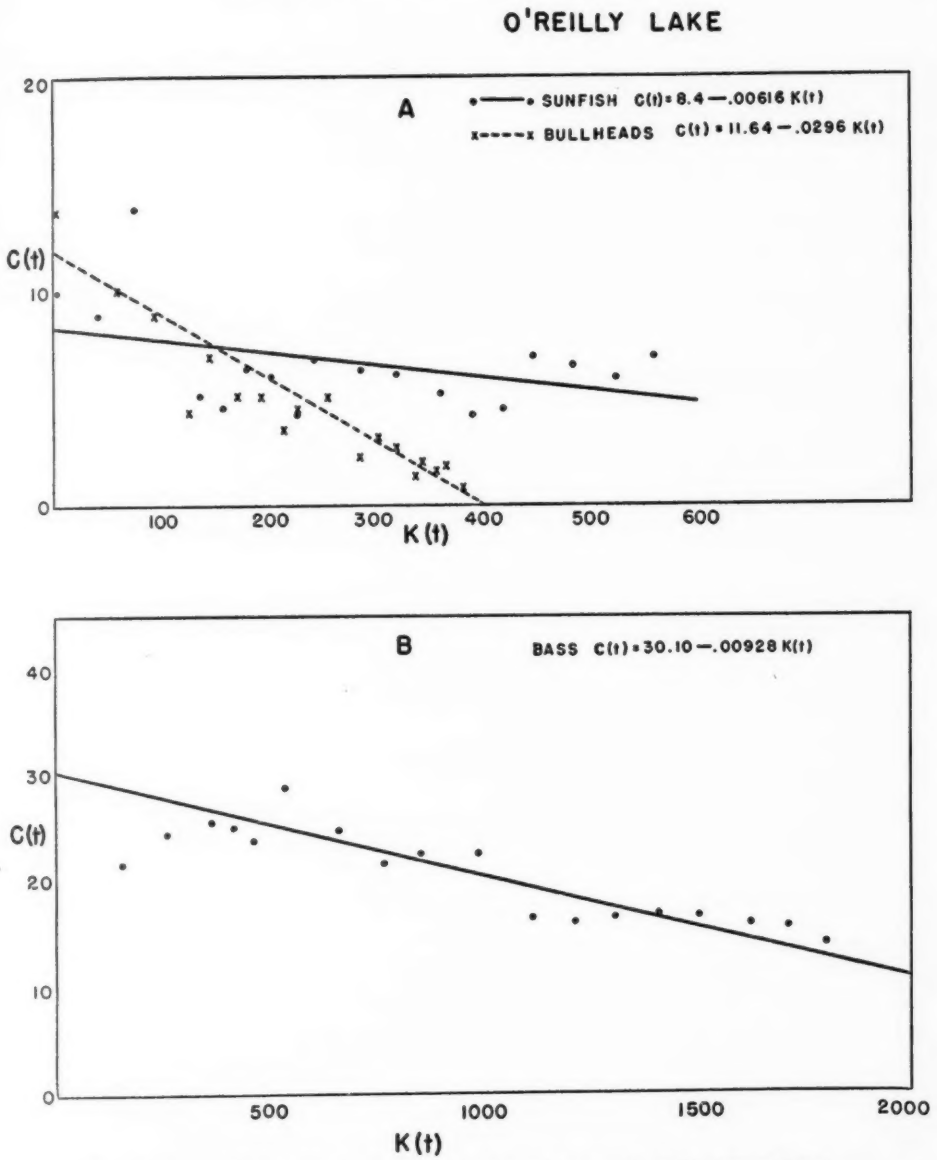

Fig. 3. Accumulated catch plotted against catch per unit effort, O'Reilly Lake.

an indication that estimates of total populations formed on this basis are minimal, and dependent upon the amount of shoreline fished.

Indications derivable from Table 4 on bullheads are that fishing effort which is exerted effectively on a bass population is also exerted on any bullhead population which is present. The close conformity to the expected pattern of

TABLE 4.—SUMMARY

	O'REILLY LAKE	DAVERN LAKE	LITTLE SILVER LAKE
Bass	C(t) = 30.10 − .00928 K(t) Conformity good N(o) = 3380	C(t) = 11.6 − .0297 K(t) Conformity good N(o) = 385	C(t) = 15.6 − .0147 K(t) Conformity good N(o) = 1060
Bullhead Catfish	C(t) = 11.64 − .0296 K(t) Conformity Good N(o) = 400		C(t) = 18.9 − .01217 K(t) Conformity Good N(o) = 1575
Sunfish	C(t) = 8.4 − .00616 K(t) Conformity poor N(o) = 1360	C(t) = 46.4 − .00132 K(t) Conformity poor N(o) = 35000	C(t) = 7.34 − .00626 Conformity poor
Rock Bass		C(t) = 8.5 − .00283 K(t) Conformity good N(o) = 300	
Suckers			C(t) = 6.94 − .0199 Conformity good N(o) = 660
Perch		C(t) = 28.8 − .0228 K(t) Conformity good N(o) = 1500	C(t) = 7.64 − .00245 K(t) Conformity poor

the plot for bullheads in Figs. 2 and 3 indicate that the fishing effort was acting to reduce the population of these fish, at least in the area where fishing was conducted. The consistent fall in catch also suggests that migration was minimal into the depleted areas. The drastic reduction in numbers in O'Reilly Lake (Fig. 3A), and the figure of 400 for total population at the beginning of the experiment, indicate that in this lake the population was very susceptible to the method of fishing. The estimate for population is, of course, applicable only to the area of water fished, or about 70% of the total shoreline. If, as these data suggest, these fish are closely associated with smallmouth bass, the substantial number of bass taken may indicate that these lived to some extent at the expense of the bullheads, the latter surviving in reduced numbers, in consequence.

In none of the lakes investigated, was there any relation between catching of sunfish and of bass. This might be explained on the supposition that either the main portion of the sunfish population was outside the area fished by the trap nets, and those which were taken were stragglers moving into the area of the trap nets by chance, or on the supposition that the sunfish population is much less local than the bass population, moving in great numbers into the fished areas. In either case, a much greater population of sunfish is indicated than is suggested by the catch effort data. The same applies to the rock bass population of Davern Lake.

In Little Silver Lake the sucker population conforms very well to the pattern believed to be produced by an effective fishing effort. An environment similar to that occupied by bass and catfish is indicated with a minimum of movement into heavily fished areas.

In the case of perch in Davern Lake (fig. 1A), a good conformity is noted, while in Little Silver Lake (fig. 2B) there is none. This is probably ascribable to the different conditions en-

countered in the two lakes, one being rather rocky, and possibly with a minimum of environment suitable for this species, while the other was shallower and may have offered more environment to them. In the first case a limited population confined to limited areas would be quickly fished down while in the latter case, there might be more movement which would obscure the effects of the fishing.

Certain general conclusions may be offered on the basis of the above data.

1. An intense fishing effort applied against a given species of fish reduces materially the population present, and this reduction is reflected in a reduced return for the fishing effort expended.

2. Certain species, in this case smallmouth bass, bullheads, and catfish, occupy fairly limited ranges at the time of year when this experiment was conducted. Drastic reduction of the population in one area of the lake was not followed by migration into that area from outside. This means that catchability does not decrease as the population becomes less, but, for the areas in which fishing was conducted, a straight line relationship describes the falling off of the catch as the population is reduced.

3. The reduction in return is related to the total population and this may be estimated from these returns, at least for the area where fishing is conducted. These estimates are minimal and are dependent on the amount of the environment occupied by the species in question which is covered by the fishing effort.

4. The degree of conformation to a pattern reflected in the catch of two species taken by the same effort may indicate an over-lapping of habitats for the two species concerned. In this case it appears that smallmouth bass and bullhead catfish are affected by the same fishing effort, and may occupy the same environment at the season of the year when fishing was conducted. Since they are both carnivorous fish, it may be that they compete both for space and food.

5. Fishing effort which is exerted against smallmouth black bass in such a way as to reduce materially the population of these fish is not effective in reducing a population of sunfish or rock bass which is inhabiting the same body of water. This may be due to the fact that the main bodies of the populations of all these species do not occupy the same habitats in the late summer and early fall. If this is so, then competition between bass and sunfish for food and space is minimal. The alternative conclusion to this is that rock bass and sunfish have a much greater tendency to move into an area where the population is depleted than do bass or bullheads. In either case, much more prolonged fishing would be necessary to reduce the population of these species materially.

The suggestions presented above are advanced as a possible interpretation of the observed data. Since a fishing program such as has been described is operated every summer by the Ontario Department of Lands and Forests, and will be expanded, it is believed that any information derivable from the catch effort statistics obtained will be expanded to cover more bodies of water each year, and in addition it may be possible to check some of these results on lakes that have been fished

when they are cropped again in future years.

The author wishes to thank Dr. W. J. K. Harkness, of the Ontario Department of Lands and Forests, Dr. D. B. DeLury, of the Ontario Research Foundation, and Dr. F. E. J. Fry, of the University of Toronto, for a critical reading of the manuscript, and Mr. J. S. Hazzard of the Ontario Department of Lands and Forests for his work in the field.

REFERENCE CITED

DELURY, D. B. 1947. On the estimation of biological populations. Biometrics 3(4): 145–167.

Accepted for publication February 8, 1950.

BRIEFER ARTICLES

STANDARD TERMINOLOGY FOR DEER TEETH

The purpose of this paper is to clarify terminology used by wildlife technicians in describing deer incisors, canines, and premolars.

Some wildlife literature refers to all incisiform teeth as incisors and some papers refer to the outer or corner incisors as canines. Differences also occur in terms used to designate the premolar adjacent to the molars. Some papers refer to this tooth as P^3 while others use the term P^4 in referring to the same tooth.

Severinghaus (1949) in his recent paper on tooth development in white-tailed deer, quotes Cowan (1936) as follows: "at the age of 15 months the median root of the deciduous fourth premolar is being dissolved." Severinghaus then states that "In this study only three deciduous premolars were found." On the same page Severinghaus further indicates that the teeth he calls incisor "corners" are the same as the incisor-like canines referred to by Cowan.

Other recent deer publications mentioning teeth use approximately the same terminology as Severinghaus; see McLean (1936) and Hunter (1947) on mule deer, and Cahalane (1932) and Park and Day (1942) on whitetails. It seems, therefore, appropriate to indicate the reasons for at least understanding the terminology employed by Cowan.

The complete original dentition for placental mammals is believed to have "consisted of three incisors, one canine, four premolars, and three molars in each half jaw and is written thus: $I\frac{3}{3}$, $C\frac{1}{1}$, $P\frac{4}{4}$, $M\frac{3}{3}$." (Hyman, 1942, and Scott, 1913)

In the fields of paleontology, taxonomy and comparative anatomy, the tooth formula for *Odocoileus* is generally written as follows: $I\frac{0}{3}$, $C\frac{0}{1}$, $P\frac{3}{3}$, $M\frac{3}{3}$ (Wortman, 1886). But individual teeth are designated in this manner:

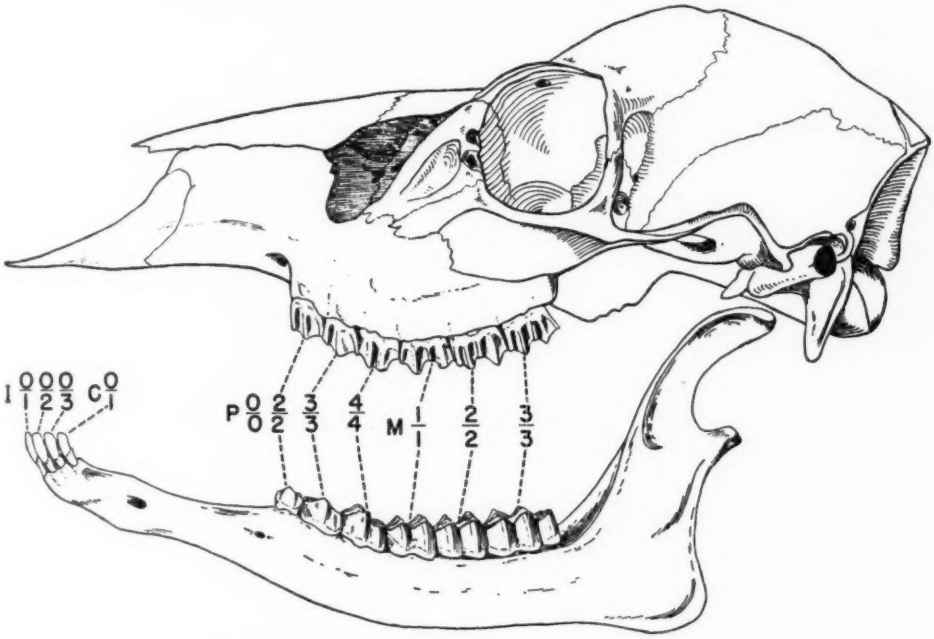

FIG. 1. Skull of adult mule deer (*Odocoileus hemionus*) showing terms used in designating each tooth.

Incisors $\frac{0}{1}\frac{0}{2}\frac{0}{3}$, Canines $\frac{0}{1}$, Premolars $\frac{0}{0}\frac{2}{2}\frac{3}{3}\frac{4}{4}$, Molars $\frac{1}{1}\frac{2}{2}\frac{3}{3}$ (Figure 1).

In discussing the evolution of artiodactyl dentition, Loomis (1925) indicates that the lower canine has been modified to an incisor in form and function, while the first premolar takes on the form and function of a canine. This character is common to early camels, deer, sheep, cattle and antelopes for example and is indicative of common origin. Although some artiodactyls still retain the first premolar (pigs, for example) in the deer this tooth is missing and only premolars 2, 3 and 4 remain.

Paleontological papers dealing with deer provide ready examples of this standard terminology in use. Matthew in discussing the phylogeny of the American Cervidae (1908) refers to *Odocoileus* premolars as No's 2, 3, and 4 as does Frick (1937). Stirton (1944), in commenting on relationships of the cervoid family Palaeomerycidae, includes a labelled diagram of an ungulate upper tooth row and designates the last premolar as No. 4.

Taxonomic papers like that of Cowan (1936) and comparative anatomical works like that of Colyer (1936) commonly follow this terminology in referring to canines and fourth premolars.

American mammalogists differ in their designations. The majority of them like Burt (1946), Cory (1912) and Hamilton (1942) follow the accepted system as described above, while other authors writing in a more popular vein, like Anthony (1928) and Seton (1909), refer to all incisiform teeth as incisors.

Persons unfamiliar with the terminology described above would admittedly be puzzled by the use of "canine" in describing the corner incisiform teeth. Even more puzzling would be the designation P^4 for the last premolar, third from the front. As I see it, the terminology used by wildlife technicians in running hunter check stations using temporary local help, or the terms used in conversing with ranchers, will continue to vary from one section of the country to another.

But, since the wildlife field stems from older and more basic fields of comparative anatomy, mammalogy, paleontology and taxonomy, trained wildlife technicians should continue with the accepted terminology currently in use in these parent fields rather than adopt more superficial designations even though the latter terms are perhaps more widespread among present wildlife field men.

LITERATURE CITED

ANTHONY, H. E. 1928. Field book of North American mammals. G. P. Putnam's Sons, N. Y., 673 pp.

BURT, W. H. 1946. The mammals of Michigan. Univ. Mich. Press, Ann Arbor, 288 pp.

CAHALANE, V. H. 1932. Age variation in the teeth and skull of the white-tailed deer. Cranbrook Inst. Sci., Pub. **2**: 1–14.

COLYER, F. 1936. Variations and diseases of the teeth of animals. J. Bale, sons E. Danielsson, Ltd., London, 750 pp.

CORY, C. B. 1912. The mammals of Illinois and Wisconsin. Field Mus. Nat. Hist., Zoological Ser. 11, Publ. **153**: 1–502.

COWAN, I. McT. 1936. Distribution and variation in deer of the Pacific Coastal Region of North America. Calif. Fish and Game, **22**(3): 155–246.

FRICK, C. 1937. Horned ruminants of North America. Bull. Amer. Mus. Nat. Hist. **69**: 1–669.

HAMILTON, W. J. 1939. American mammals. McGraw-Hill Inc., N. Y., 434 pp.

HYMAN, L. H. 1942. Comparative vertebrate anatomy. Univ. Chicago Press, Chi., 544 pp.

HUNTER, G. N. Undated (1947). Physical characteristics of Colorado mule deer in relation to their age class. Publ. Colorado Game and Fish Dept., 1–38.

LOOMIS, F. B. 1925. Dentition of Artiodactyls. Bull. Geol. Soc. Amer., **36**(4): 583–604.

McLEAN, D. D. 1936. The replacement of teeth in deer as a means of age determination. Calif. Fish and Game, **28**(1): 43–44.

MATTHEW, W. D. 1908. Osteology of *Blastomeryx* and phylogeny of the American Cervidae. Bull. Amer. Mus. Nat. Hist., Art. 27, **24**: 535–562.

PARK, B. C. and B. B. DAY. 1942. A simplified method of determining the condition of white-tailed deer herds in relation to available forage. U.S.D.A. Tech. Bull. No. 840.

SCOTT, W. B. 1913. A history of land mammals in the Western Hemisphere. The Macmillan Co., N. Y., 693 pp.

SETON, E. T. 1909. Lives of game animals. Doubelday and Doran and Co., N. Y. 3: 1–780.

SEVERINGHAUS, C. W. 1949. Tooth development and wear as criteria of age in white-tailed deer. Jour. Wildl. Mgt., 13(2): 195–216.

STIRTON, R. A. 1944. Comments on the relationships of the cervoid family Palaeomerycidae. Amer. Jour. Sci. **242**: 633–655.

WORTMAN, J. L. 1886. The comparative anatomy of the teeth of the vertebrata. American System of Dentistry **1**: 351–515.

—THANE RINEY, *Museum of Vertebrate Zoology, University of California, Berkeley.*

Accepted for Publication March 25, 1950.

A METHOD OF COLOR MARKING YOUNG WATERFOWL[1]

Few studies of young game birds in the field have been concerned with recognizably marked individuals. This is due mainly to the difficulties involved in capturing the young and in devising means of identification which would not hamper their activities.

J. B. Low (1945, Ecol. Mono. **15**(1): 35–69) was able to mark the young of the Redhead (*Aythya americana*) by gluing colored feathers to their heads.

S. A. Nordberg (1942, Ornis Fennica **19**(3): 73–80) working in a rocky island habitat with eiders, scoters and mergansers was able to approach to within 3 or 4 meters of incubating hens and squirt dye on them. When the broods hatched, the marked hens identified them for the rest of the season. However, under most conditions it is difficult to mark hens without causing excessive desertion and predation.

Lyle K. Sowls suggested marking the young birds by the use of dyes injected into the eggs during incubation. Although the idea is not new and has apparently been applied commercially, no information was available concerning the methods used. Therefore, work was done during the spring and summer of 1948 at the Delta Waterfowl Research Station and during the early spring of 1949 at the University of Minnesota to develop a workable method.

In the course of these experiments, 134 chicken eggs, 129 wild duck eggs and 3 ruffed eggs were injected while 23 dyes were given preliminary tests and a technique of injection was developed. Experiments showed that toxicity to the embryo and ability to color the down must be considered in choosing a dye. Resistance to fading was also considered an important factor but was not tested due to lack of time. Although it is stated by the Warner-Jenkinson Manufacturing Company (Booklet C-41; Certified Food Colors, p. 34) that some dyes are harmed when exposed to high temperatures, no difficulty was encountered in this respect.

The preliminary tests showed that most of the dyes lacked one or both of the necessary qualifications; while those listed in Table 1 were apparently satisfactory.

TABLE 1.—DYES SHOWN TO BE SATISFACTORY BY PRELIMINARY TESTS

Dye	Color	Concentration used (grams/ 100 ml. water)
Amaranth*	cherry red	11.5
Brilliant Blue FCF	light blue	15.9
Croceine Scarlet MOO	scarlet	3.8
Fast Green FCF	bright green	14.6
Patent Blue*	light blue	13.8
Ponceau SX	scarlet	6.0

* These two dyes were mixed in the ratio of 1:1 to produce purple.

Croceine Scarlet and Patent Blue were obtained from the National Aniline Division of the Allied Chemical and Dye Corporation, Chicago 10, Ill. The others are certified food colors produced by the Warner-Jenkinson Manufacturing Company, St. Louis, Mo.

These dyes were dissolved in distilled water slightly below their saturation point so that they would not become supersaturated due either to variations in the solubilities of different samples or to cool weather.

[1] Paper Number 2550, Scientific Journal Series, Minnesota Agricultural Experiment Station.

During the summer of 1949, field studies of waterfowl brood movements were made with the aid of this technique. Fast Green FCF and Ponceau SX were used with satisfactory results. Croceine Scarlet was used in one nest but was discontinued in favor of Ponceau SX. A mixture of Amaranth and Patent Blue was tried in an effort to obtain a purple color. Since no broods colored with this mixture were seen, it cannot be evaluated.

The dyes were carried in 60 milliliter serum bottles with soft rubber caps. The bottles with caps set loosely on top were sterilized together with the packaged syringes and needles in an ordinary home pressure cooker at 15 lbs. pressure for 20 minutes. After removal from the cooker, the bottles were firmly stoppered. The syringes have a capacity of 2 milliliters and were wrapped disassembled first in cotton cloth and then in wrapping paper. The 22-gauge hypodermic needles were carried in 100 milliliter blood tubes plugged with cotton. Under field conditions, equipment packed in this manner should remain sterile for at least 3 months. A 20-gauge hypodermic needle run through a rubber stopper with the point projecting about 1/8 inch made a very efficient egg drill when pressed against the shell and rotated slightly. The small bottle contained 1 part mercuric chloride in 1000 parts of 50% ethyl alcohol used as a disinfectant, while the dropper bottle contained collodion used to seal the eggs after the injection. A wooden rack was a great aid in carrying the eggs and holding them steady during the treatment.

A cardboard cylinder 1¼ inch in diameter and 8 inches long cut at one end to fit the contour of an egg and at the other end to fit close to the eye of the observer was used in estimating the stage of incubation of the embryos. When an egg was placed over one end and the apparatus was held up to the sky it served as a candler. Since visibility through the shell was insufficient to allow inspection of the development of the embryo itself, the size and shape of the air cell was used as the criterion of the stage of development. This method as applied to pheasant eggs is described in detail by Westerkov (1950, Jour. Wildl. Mgt. **14**(1): 56–67). Since no actual measurements of air cell diameter were taken and estimates were made only by eye, ageing was not absolutely accurate. However, the cylinder was easily carried when nest hunting so that nests were aged when found, obviating the necessity for an extra visit and lessening the chances of predation and desertion. In cases where the age was in doubt, one egg of the clutch was opened and the embryo itself inspected.

When a clutch was to be marked, the eggs were placed air cell down in the rack and removed some distance from the nest in order to minimize disturbance of the cover. A point about 1/2 inch from the small end of the eggs as well as the drill and serum bottle cap were daubed with mercuric chloride, and a hole was punched in the shell.

When unpacking and assembling the syringe and needle, care was taken that they did not become contaminated by contact with foreign objects.

After the syringe had been filled, the excess air was driven out of it. A small bubble was retained as it was more easily visible than the plunger of the syringe and served as an aid in measuring the dosage. In order to avoid injury to the embryonic membranes, only the tip of the needle was pushed through the hole in the shell and the dye was injected slowly in order to prevent overflow. After the holes had been sealed with collodion, the eggs were returned to the nest and covered with down.

Thirty-six eggs were injected with Fast Green FCF and fifty-six with Ponceau SX in varying amounts and at different stages of incubation. Seventy-seven of these were in successful wild duck nests while fifteen were checken eggs hatched in an incubator. A study of the results obtained furnishes the basis for the dosages suggested in Table 2.

TABLE 2.—OPTIMUM DOSAGES PER EGG OF TWO DYES USED IN THE FIELD

Species	Approximate egg weight	Dosage of Fast Green FCF	Dosage of Ponceau SX
Blue-winged Teal	28 grams	0.12 ml	0.19 ml
Green-winged Teal	29 grams	0.12 ml	0.19 ml
Shoveller	39 grams	0.16 ml	0.26 ml
Baldpate	43 grams	0.18 ml	0.29 ml
Pintail	43 grams	0.18 ml	0.29 ml
Gadwall	47 grams	0.19 ml	0.31 ml
Mallard	54 grams	0.22 ml	0.36 ml
Redhead	62 grams	0.25 ml	0.41 ml
Canvasback	64 grams	0.26 ml	0.42 ml

These dosages are calculated on the basis of weight of dye per unit of egg weight which compensates for variation in the size of embryos at hatching.

The weights of eggs were calculated with the aid of W. H. Bergtold's (1929, Auk **46**: 466–473) formula which states that $W = 11/21 (LB^2)S$ where W = the weight of the egg in milligrams, L = its length and B = its breadth in millimeters and S = its specific gravity (1.075 for the fresh eggs of birds with precocial young). The measurements were taken from Kortright (1943, The Ducks, Geese and Swans of North America).

The dosages shown are equivalent to dry weights of 0.6 grams of Fast Green FCF and 0.4 grams of Ponceau SX per kilogram of egg weight. The former is best injected 2 to 11 days, and the latter 2 to 8 days before hatching.

The results are considered in the light of coloration of the young and embryo mortality. Although data are few and conditions were variable necessitating individual consideration of each case rather than statistical treatment, these amounts are apparently well above the minimum necessary to produce good color and are apparently not great enough to cause excessive embryo mortality. Injections made too early in incubation show a tendency to produce dull colors and are apparently more lethal than those made later. If injections are made too late, the dye does not become distributed over the embryo and coloration may be spotty and dull.

Accuracy of dosage is difficult to achieve under field conditions and is not necessary since variations of over 40% from those shown have produced good results.

The effect of the technique on desertion and predation appeared to be slight. Nineteen clutches were marked and twelve (63%) hatched successfully. The high success of these clutches is probably due to the fact that they were not colored until well into incubation when they were past the critical period for desertion and predation.

Although there was an increase in embryo mortality due to the treatment, it was not great. Eleven successful nests treated with Fast Green FCF or Ponceau SX contained a maximum of 79 fertile eggs. Examination of the shell membranes or observation of the young showed that at least 55 (70%) had hatched successfully.

Seven of the broods marked with Fast Green FCF and Ponceau SX were observed after hatching and were well colored. Another clutch dyed with Croceine Scarlet was poorly colored.

The duration of color of 5 broods observed after leaving their nests is indicated in Table 3.

TABLE 3.—DURATION OF COLOR ON FIVE MARKED BROODS

Species	Dye used	Duration of color
Canvasback	Fast Green FCF	23 days
Canvasback	Ponceau SX	32 days
Baldplate	Ponceau SX	23 days*
Pintail	Ponceau SX	16 days**
Redhead	Croceine Scarlet	30 days

* Color still bright when last observed.
** Seen only once. Color good.

In the case of Fast Green FCF, the loss of color was apparently due to rapid fading. Ponceau SX faded much less rapidly and color loss was due as much to molting of the down and growth of feathers as to fading of the dye. None of the young were captured to determine whether or not close inspection would show traces of color when it could no longer be seen in the field. However, examination of colored birds raised in the Delta hatchery showed that patches of color remained under the wings when it was no longer visible from a distance. Some of these birds retained color up to seven weeks after hatching.

Observations of colored young both in the Delta hatchery and in the wild failed to disclose any abnormality in their own behavior or in their relationships with other birds.

This marking technique, while still rather crude, provides a practical method of distinguishing individual broods during the early stages of their development and enables the observer to record their movements.—CHARLES D. EVANS, *University of Minnesota, St. Paul, Minnesota.*

Accepted for Publication March 28, 1950.

A SIMPLE TECHNIQUE FOR DEN-USE STUDY

The study of mammals using ground dens has often been aided by telltale hairs left by occupants of a den on sharp stones or roots near the entrance. The hairs thus caught are often fragmentary and not always sufficient in number to provide the characteristics necessary for identification. Small hair-catching devices that are easily inserted into the entrance of a den have proved of value in determining the species of animal using the den.

Three successful hair-catching devices were developed at the University of Michigan and were tested on an area of land about four miles east of Ann Arbor. Two other records of similar devices have been found. Steuwer used "wire combs" with some success on artificial raccoon dens in Michigan. Drahomer, in an unpublished thesis at the University of Michigan used wire devices in ground dens with reported success. Figure 1 illustrates those here described.

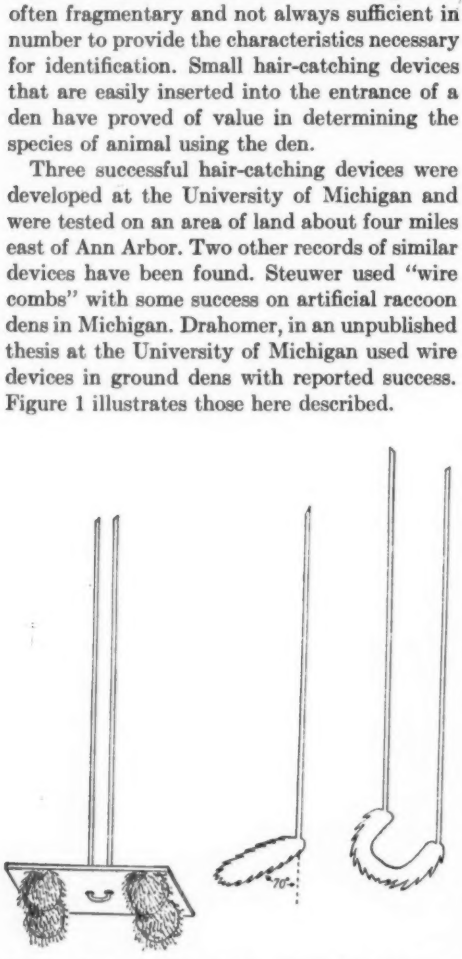

FIG. 1. Hair-catching devices described in text.

Of the three successful devices, the most satisfactory one was made with the seed heads of burdock (*Arctium* sp.). Four or six seed heads were glued to a thin piece of plywood measuring 1.75″ x 0.75″. A satisfactory waterproof glue is "Hold All" brand liquid cement. A U-shaped piece of wire (9–12 ga.) is inserted through two holes in the wood. The head of the device may be waterproofed by dipping it in hot paraffin. The pulling quality of the barbs is not impaired by the paraffin. It also serves to bind the burdock seed head together as well as giving it complete protection against soaking by ground water. The construction time, save for the drying of the glue, is less than ten minutes.

Insertion of the device in the den is simple. The wire ends are pushed upward into the earth with the burdock seed heads in range of the animals body. In dens with a large entrance, the device will either need a longer piece of wire or it will have to be shifted to the side or bottom of the den.

Burdock devices must be wrapped in tissue paper when carrying a number of them into the field. This protects them from damage as they catch on one another if not wrapped.

Successful results were also obtained from two wire devices which may be used when burdock seed heads are not available. The two wire devices are essentially the same in principle and vary only in form. The first is made of a straight piece of wire (9–12 ga.), 6″ or 7″ long. About one inch of one end is flattened thin with a ball-peen hammer. With small tin snips, sharp barbs are cut along the flattened edges about an eighth of an inch apart. The barbed area is bent at a 70° angle to the main shaft. When inserted in the den, the barbs should point out towards the entrance.

The other wire device differs only in that a U-shaped piece of wire is used. The U end is flattened and is likewise bent out at a 70° angle.

Of more than fifty experimental devices set, 81% had successful hair catches. Many were set in dens later determined to be entirely abandoned. Observations of den usage showed that few ground dens were used before cold weather set in. The number of dens being used increased proportionally with the decrease in temperature. Early in the fall, 26% of the dens observed were determined to be in use. Temperatures were warm during the day and did not drop below 50° F at night with a few exceptions. Weekly checks showed a progressive increase in usage and on January 1, 75% of the dens observed were in actual use. This last figure was determined partly by tracks in fresh snow. Temperatures had been near 0° F.

Five species of animals left hair samples in devices in the following order of abundance: rabbit, skunk, opossum, weasel, and fox. It is felt that other species of mammals would undoubtedly leave successful catches of hair in the devices.

The identification of the various hairs is a problem of considerable difficulty. The hair of some animals can be identified easily without aid of magnification. Others are similar in appearance with or without the aid of a microscope. Neither hair keys nor hair drawings allow for the tremendous variation that occurs in a single species or even on a single hair. Hair keys are decidedly too time-consuming to be practical where many hair samples are to be examined.

A thorough familiarity with hair variations is essential to their identification. Examination of known specimens will reveal the many variations. From these observations, drawings should be made.

A great aid in the study of hair is the microscopic observation of the hair scales. Because they are seen with difficulty on the hair itself, impressions can be made in plastic material that will harden rapidly leaving accurate impressions in it. Various methods have been developed but most of them require much preparation time. A fast simple method involves the use of clear fingernail polish.

On a clean microslide, a thin streak of polish is made. The hair is placed in the liquid polish, the thumbs holding the ends of the hair so that it is tightly stretched parallel to the long axis of the slide. This results in a fairly even hair impression. The hair is held thus until the polish has dried, and is then removed. The scale impressions may be clearly seen under the microscope. Variation again is a restraining factor in identification. However, a combination of characteristics may be used to identify some species.—RICHARD J. HARTESVELDT, 1057 *Revere Court, Willow Run, Michigan.*

Accepted for Publication March 29, 1950.

GREY SQUIRREL PARASITISM BY HEEL FLY LARVAE

The larvae of heel flies are common parasites in both wild and domestic animals. Those found in grey squirrels in the Southeastern States belong to the genus *Cuterebra*. Heel flies affecting cattle in this region belong to a different genus and the larvae emerge in late winter or early spring, while the *Cuterebra* in squirrels emerge in early fall.

This parasite is of particular interest to game managers due to heavy infestation in the squirrels of some Southeastern States. Infestation is heaviest in hilly localities having well-drained, sandy soils. Such soils may form a better medium for pupation than wetter or heavier types. Animals killed with well-developed larvae in them are considered unfit for food and are thrown away, resulting in considerable waste of valuable game. These larvae, called "wolves" or "warbles" by hunters, are repulsive grubs, in the final larval stage greyish black in color, about one inch in length and one-third inch in diameter, tapering at each end. Infested squirrels may contain from one to five, usually in the back, neck, or shoulders. They cause large, angry sores. These sores remain for a short time after the parasites emerge but tend to heal rapidly.

Squirrel seasons opening before the larvae have left the animals or the sores have had time to heal result in much waste that could be avoided by a later opening date. In addition to this waste, much of the pleasure of hunting is spoiled when a high percentage of the animals are covered with the ugly sores.

Public grey squirrel hunts have been held on the Wheeler National Wildlife Refuge, located in the Tennessee Valley of north Alabama, for the past six seasons. Primarily a waterfowl refuge, the area is opened to squirrel shooting for a short time each fall to hold these animals to a normal population level. The hunts are closely supervised and careful records have been kept on the amount of heel fly infestation each year. Since hunt dates have varied, correlation between the dates and the

amount of parasitism is interesting. Table 1 shows the data on hand:

TABLE 1.—INCIDENCE OF CUTEREBRA IN GREY SQUIRRELS

Date of Hunt	Total Bag	Number Parasitized	Percentage of Parasitism
Oct. 1–15, '46	2128	151	7.14
Oct. 15–21, '47	1667	36	2.16
Oct. 17–22, '49	3047	44	1.44
Oct. 18–23, '48	1891	7	0.40
Nov. 20–Dec. 13, 1945	1097	1	0.00
Dec. 1–20, '44	611	0	0.00

While these data are limited and may apply directly to only north Alabama and similar latitudes, they show a tendency of the infestation to drop sharply as the fall progresses. It seems probable that a hunt held November 1 would avoid any appreciable waste. The emergence of these parasites proceeds rapidly during the second or third weeks of October and most of the squirrels taken in late October and classified as parasitized did not contain larvae, although the unhealed sores remained. In some localities subject to heavy heel fly infestation, postponing the opening date of the grey squirrel season until after the first heavy frosts or light freezes occur will increase the attractiveness of the sport and eliminate much needless waste of good table meat.—THOMAS Z. ATKESON and LAWRENCE GIVENS, *Box* 1643, *Decatur, Ala.* Accepted for Publication June 12, 1950.

FALL FOOD HABITS OF THE CLAPPER RAIL IN GEORGIA

This report summarizes findings from 669 Clapper Rail, *Rallus longirostris*, gizzards collected in coastal Georgia during October and November, 1947. Most specimens were probably of the Wayne's Clapper Rail subspecies (*Rallus l. waynei*) but it seems very probable that some individuals of the Northern Clapper Rail (*Rallus l. crepitans*) may also have been included. The winter range of the latter subspecies extends into Georgia.

Study materials were obtained through excellent cooperation from residents in the area. Preceding the 1947 hunting season, four-ounce bottles containing formaldehyde were distributed to nearly all the boat-landing owners along the entire coast of Georgia. These individuals agreed to preserve gizzards they obtained from local hunters and to record the date, place, and height of tide at time of collection on each bottle of specimens.

All the birds from which gizzards were removed were killed at the highest stage of tide. This would imply that the birds probably had not fed for at least two hours before they were shot. Consequently, one might have expected to find considerable uniformity in the gizzards as to quantities of foods present and their state of digestion. This was not the case. Food volumes in the series of gizzards ranged from zero to 6.2 cc. and the stage of digestion varied from finely-ground residues to undigested whole bodies of insects and crabs.

Of the 669 gizzards collected 118 were immediately excluded from tabulations because they proved to be completely or nearly empty. Two hundred sixty-seven additional gizzards that were less than one-quarter full were excluded eventually from the data after trial tabulations showed that their inclusion seemed to cause significant distortion of results. The practice of excluding gizzards with very limited food quantities conforms with policies set forth by Martin, Brown, and Gensch.

Figures on percentage by volume of the particular kind of food eaten were obtained by estimating the percentage in the aggregate volume of all foods consumed. Data on percentage by occurrence are based on the number of times (out of a possible 284) that items occurred in the series.

Sixty-one kinds of food were found in the Clapper Rails gizzards but most of these occurred in small quantities—traces or fractions of 1%. Ten of the 61 foods constituted 97%, volumetrically, of the whole diet. In Table 1, these ten more-important foods are recorded with their percentages and the minor items are listed without percentage figures.

This compilation indicates that the Clapper Rails' diet in Georgia during the late fall is

TABLE 1.—FOOD ITEMS IN 284 CLAPPER RAILS

	% By Vol.	% By Occ.
PARASITIC WORMS, Ascaridae	traces	
SNAILS		
Littorina irrorata	14	37
Melampus sp., *Nassarius obsoleta*, *Polygyra* sp.	traces	
CLAMS, Molulidae	traces	
CLAM WORMS, *Nereis* sp.	traces	
SHRIMPS, *Peneus setiferus* and *Palemonetes* sp.	traces	
CRABS		
Sesarma cinereum	33	58
Sesarma reticulatum	15	37
Sesarma sp.	6	17
Uca sp. (with traces of *Uca pugilator*)	14	45
Eurytium limosum	3	17
Panopeus herbstii	1	1
Unidentified crabs	2	7
Callinectes sp. and Pinnotheridae	traces	
INSECTS		
Phalaenidae (—Noctuidae)	6	12
Other insects, listed below	3	—
Undet. Orthopetera, undet. Tettigoniidae, undet. Acrididae, *Neoconocephalus triops*, undet. *Gryllidae*, *Gryllus assimilis*, *Scapteriscus* sp., *Schistocerca americana*, *Melanoplus* sp., Blattidae, undet. Dermaptera, *Anisolabis* sp., Hemiptera, Pentatomidae, *Acantocephala declivis* and *A.* sp., *Leptoglossus phyllopus*, undet. Lepidoptera, Diptera, *Hylobius pales*, *Sphenophorus* sp., undet. Hymenoptera, undet. Ichneumonidae, *Gambrus bituminosus*, *Crematogaster* sp., *Apis mellifera*		
SPIDERS, *Lycosa* sp. and *Clubiona* sp.	traces	
FISH, Poeciliidae and *Fundulus* sp.	traces	
PLANTS, *Spartina*, mainly *S. alterniflora*	traces	
	97%	

composed almost wholly of animal foods. Cordgrasses (*Spartina*) are the chief plant components of the marshes yet their seeds constituted only traces in this series.

Crabs of several species are common in the tidal marshes and they are the principal source of the rails' food—totalling nearly 75%. The Square-backed Fiddler Crabs (*Sesarma*) occur typically in soft mud along the sloughs and creeks and are more inclined to be solitary in habit than the Common Fiddler Crbas (*Uca*). The latter are found abundantly on higher ground in the marsh. The Square-backs seem to be the less abundant of these two yet the data show that they are eaten to a much greater extent. Crabs of the genera *Eurytium* and *Panopeus* are fairly abundant in the marsh. The latter seems to be the less common of the two.

The snail *Littorina irrorata* is common over all of the marsh. It is ingested whole by the rails—one bird having as many as 48.

Insects, though plentiful in the marshes and though taken commonly in small amounts, would be of little apparent importance to the Clapper Rail except for the Cutworm Moth, family Phalaenidae. Spiders and small fish constituted an almost negligible part of the diet.

The author is grateful to Dr. C. W. Watson, Mr. Thomas H. Jones, Mr. Louis O. North, and members of the Patuxent Research Station who made the investigation possible and aided in carrying it to completion.—JOHN ONEY, *Georgia State Game & Fish Commission, Pittman-Robertson Project 9R, State Capitol, Atlanta, Georgia.* Accepted for Publication April 7, 1950.

OBSERVATIONS ON THE FALL AND WINTER FOOD OF BOB-WHITE QUAIL IN KENTUCKY

As a part of the Kentucky Quail Investigation (Pittman-Robertson Project 6R) the writer, assisted by various local sportman's organizations, made a collection of crops and gizzards of the Bobwhite Quail (*Colinus v. virginianus*) from three widely separated counties in Kentucky.

Ninety-eight birds were collected in Calloway County, located in the westernmost section of the state. Eighty-five birds were collected in Warren County, situated in the south-central portion of Kentucky. One hundred and forty-eight birds were taken in Pulaski County, located in the western edge of mountainous eastern Kentucky.

The exact date of collection of the birds is unknown; it is known, however, that all were taken between November 24, 1941, and January 6, 1942. Credit for all analyses is due the Fish and Wildlife Service.

The Bobwhite Quail is known to feed on a great variety of plant and animal matter; the following tables present only those items making up more than two percent by volume, of the food eaten. This arbitrary percentage was selected to limit this note to dominant items in the diet. The results of crop and gizzard analyses are not separated.

TABLE 1.—STOMACH CONTENTS OF 98 QUAIL COLLECTED NOVEMBER 24, 1941–JANUARY 6, 1942, IN CALLOWAY COUNTY, KENTUCKY

Item	Percent by Volume	Percent Frequency
Lespedeza striata	16.62	51.02
Zea mays	15.71	26.53
Lespedeza stipulacea	14.10	29.59
Robinia pseudo-acacia	7.71	22.44
Crotonopsis linearis	3.72	29.59
Sorghum vulgare	2.88	3.06
Sida spinosa	2.11	19.38
Rhus sp.	2.11	13.26

TABLE 2.—STOMACH CONTENTS OF 85 QUAIL COLLECTED NOVEMBER 24, 1941–JANUARY 6, 1942, IN WARREN COUNTY, KENTUCKY

Item	Percent by Volume	Percent Frequency
Lespedeza stipulacea	32.64	58.82
Zea mays	12.39	22.35
Desmodium sp.	10.65	34.11
Rhus sp.	10.10	58.82
Ambrosia artemesiifolia	4.48	50.59
Sassafras officinale	3.53	15.29
Galactia sp.	3.01	22.35
Diodia teres	2.98	34.11
Vitis sp.	2.98	35.29

TABLE 3.—STOMACH CONTENTS OF 148 QUAIL COLLECTED NOVEMBER 24, 1941–JANUARY 6, 1942, IN PULASKI COUNTY, KENTUCKY

Item	Percent by Volume	Percent Frequency
Ambrosia artemesiifolia	34.50	77.03
Lespedeza stipulacea	27.46	72.29
Desmodium sp.	5.87	24.33
Rhus sp.	4.94	48.65
Cornus florida	4.73	27.70
Sorghum vulgare	4.56	4.73
Zea mays	2.39	10.81
Digitaria ischaemum	2.10	11.50

On the basis of these data, it appears that the major food items of the Bobwhite Quail in Kentucky vary somewhat from section to section; this is to be expected, due to varying agricultural practices in the three sections. It is interesting to note that of the total contents of 331 stomachs, *Lespedeza stipulacea* makes up 24.83% by volume; the next four items, in order of percent by volume, are: *Ambrosia artemesiifolia*, 16.57%; *Zea mays*, 8.9%; *Desmodium* sp., 5.36%; *Lespedeza striata*, 4.69%.—ROGER W. BARBOUR, *Oglebay Institute, Wheeling, West Virginia.* Accepted for Publication November 7, 1949.

RUSSIAN OLIVE AS A WILDLIFE FOOD

Russian olive (*Elaeagnus angustifolia L.*) is being used extensively in western United States in soil and moisture conservation and wildlife habitat improvement programs. Its adaptability to a wide range of soil, moisture and climatic conditions makes it useful in windbreaks, for streambank protection and gully control, for planting in waste areas and for hedges and living fences.

In areas where it is abundant, its value as a wildlife food is well known, but little has been published on the subject. For several years the author has gathered information on the use of Russian olive by various kinds of birds and mammals. Russian olive is abundant in the vicinity of Albuquerque, New Mexico, and in adjacent portions of the Rio Grande Valley, so it has been possible to compile considerable information from this area on the use of Russian olive by wildlife and to add substantially to the data published previously.

Russian olive has most of the qualities desired in a good wildlife food plant. It grows rapidly and produces a heavy crop every year, crop failures being practically unknown, at least in New Mexico. The fruit, which matures in early fall, remains on the tree throughout the winter and, therefore, is available during the winter and early spring when other wildlife food is scarce. Fruit loosened by wind or feeding birds falls to the ground where it is available to ground feeders. The sprouting seeds are also readily eaten. Since the fruit is borne from 4 to 20 feet above the ground, it is available above deep snow.

That Russian olive is not to be classed as a starvation diet is indicated by the fact that it is eaten by many birds in the presence of grain and other food. It may be a preferred food for some species. The author has taken pheasants, adjacent to wheat and milo maize fields, that had their crops partially or entirely filled with Russian olive fruit. A covey of scaled quail was observed to walk regularly one hundred yards over open ground from a weed patch to feed on Russian olive fruit.

When available to wild ducks it is apparently readily eaten. At two sites on the bank of the Rio Grande, the ground around Russian olives was well trampled by wild ducks and all fruit within reach had been consumed.

M. S. McMurtrey, Regional Biologist, Soil Conservation Service, Lincoln, Nebraska, wrote the author as follows: "It (Russian olive) could almost be classed as a preferred food for sharp-tailed grouse as observations show they will feed on it at any time. There is an old planting of olive on the Bureau of Plant Industry Horticulture Field Station, Mandan, North Dakota. During periods of heavy snowfall, sharptails come into this planting by the hundreds."

Martin L. Burget, Colorado Department of Game and Fish, told the author that when he fed Merriam turkeys a mixture of grains and Russian olive fruit, the turkeys promptly consumed the Russian olive fruit.

The author has observed red-shafted flickers, mocking birds, western robins, evening grosbeaks, and cedar waxwings eat ten or more fruits in rapid succession.

The above observations and the following list of species known to eat the fruit of Russian olive indicate that extensive use is made of it by a wide variety of wildlife:

Reported by Van Dersal **(1938)**

1. Eastern robin (Ames, Iowa)
2. Western robin (Pullman, Washington)
3. Ring-necked pheasant (Beadle Co. North Dakota and Pullman, Washington)
4. Sharp-tailed grouse*
5. Cedar waxwing (Park River, North Dakota)
6. Hungarian partridge (Pullman, Washington and Boise, Idaho)
7. Bobwhite*
8. Western evening grosbeak (Lewiston, Idaho)
9. Valley quail*

* Location not given.

Crops from birds collected by author and analyzed by U.S. Fish and Wildlife Service

10. American magpie
11. Shufeldt's junco
12. Spurred towhee
13. Green-tailed towhee
14. Red-shafted flicker
15. Gambel sparrow
16. Brown thrasher
17. Arkansas goldfinch

18. White-crowned sparrow
19. Song sparrow

Crops examined by author

20. Gambel quail
21. Mourning dove
22. Mallard

Birds observed feeding on olive fruit by author

23. Mockingbird
24. Mountain bluebird
25. Sage thrasher
26. Pine siskin
27. English sparrow
28. House finch
29. Starling
30. Western crow
31. Brewer blackbird
32. Merriam turkey—Observation by Martin L. Burget, Colorado Dept. of Game & Fish, Durango, Colorado
33. Eastern cardinal—Observation by W. L. McAtee, Fairfax Co., Virginia

Domestic poultry—Observations by author

1. Chickens
2. Turkeys
3. Ducks
4. Pigeons

Mammals

1. Cottontail—Albuquerque, New Mexico stomach analysis by Fish and Wildlife Service
2. Fox squirrel—Van Dersal (1939)
3. Ground squirrel—Observation by J. A. Downs, Albuquerque, New Mexico

Records for species numbers 10 to 31 inclusive, and the domestic poultry were obtained in the vicinity of Albuquerque, Bernalillo County, New Mexico.

That the fruit is nutritious is indicated by the following analysis made of the fruit by the U. S. Fish and Wildlife Service:

	% Dry Wt.	% Whole Wt.
Moisture		8.84
Solids		
Crude Protein (Total N x 6.25)	10.00	9.116
Non-Protein Nitrogen	0.19	
True Protein (Amino N x 6.25)	8.86	
Ether Extract (fat)	5.94	5.415
Crude Fiber	17.15	15.634
Ash	3.24	2.953
Calcium	0.15	
Phosphorus	0.12	
Nitrogen-free Extract (by difference)	63.67	58.042
	100.00	100.00

REFERENCES CITED

VAN DERSAL, WILLIAM R. 1938. Native Woody Plants of the United States. U. S. Dept. Agr. Misc. Pub. 119.

———. 1939. Birds that Feed on Russian Olive. Auk, **56**: 483–484.

—A. E. BORELL, *Reg. Biologist, Soil Conservation Service, Albuquerque, New Mexico.*

Accepted for publication May 30, 1950.

FURTHER REMARKS ON THE MINNOW SEINE METHOD OF SAMPLING FISH PONDS

The criticism of the minnow seine method for determining the balance in fish populations as stated by George W. Bennett in his review of *Fish Ponds for the Farm* by Frank C. Edminster (1948. Jour. Wildl. Mgt., **12**(2): 215–217) has been challenged by Philip F. Allan (1950. Jour. Wildl. Mgt., **14**(1): 85–86). In that later issue Bennett further defended his criticism. In addition to the arguments presented by those men two very important items should be mentioned.

The purpose of the minnow seine method of sampling fish populations in ponds and lakes as described by H. S. Swingle (1945. Trans. 10th N. Am. Wildl. Conf., pp. 301–302) is "to know if the correct balance exists between bream and bass in any pond or lake." In a balanced pond "Sufficient bass must be present to keep the bream population under adequate control so that overcrowding will not result, but too many bass must not be present or insufficient bream will survive to replace those caught and the bass will have insufficient food for satisfactory growth." In using this method, he recommended that sampling should be done shortly after the spawning season for each

species at intervals around the pond in shallow water with a small minnow seine with 4 to 6 meshes to the inch. Interpretation of results is as follows: (1) "If insufficient bass are present to balance the pond, the bluegills may spawn only lightly in the early spring and not at all the rest of the summer, or they may not spawn until early fall; or under very severe overcrowded conditions, they may not spawn at any time. . . . If recent hatches are found during June, July, or August, it can safely be assumed that sufficient bass are present to control adequately the bluegill population and that restocking with bass would be worthless. If no recent hatches of bream can be found during this period, it is an indication that either no bass or insufficient bass are present to balance the pond, and, that restocking with bass will be necessary. Under this condition, a considerable number of 1- to 3-inch bream can usually be caught in the minnow seine. (2) "It is also necessary to determine the presence of a spring-hatch of young bass before one can be certain that neither bream nor bass are overcrowded and that both are in proper balance in the pond." (3) "The finding of a spring hatch of bass and midsummer hatches of bream indicate that a pond or lake is in relatively satisfactory balance."

Whatever may occur in Alabama, use of such information in Indiana rarely leads to an accurate picture of the degree of "balance" which may exist. For the interpretation of minnow-seine data it is necessary to assume that fish in a balanced population reproduce successfully each year. In Indiana, at least, such is not the case. In some of our experimental ponds we have found that both bass and bluegills have failed to bring off a successful brood of young even though the conditions at the time seemed to be ideal. In one pond the bass failed to reproduce successfully in 1946 but did so in 1947 whereas in another similar pond the bass reproduced successfully in 1946 but did not in 1947. In both cases the bass came from the same parent stock and were of similar size. Similar results in the success of reproduction have been observed in experimental bluegill populations. Our results so far indicate that successful reproduction in either bass or bluegills or both may occur on an average of only once in every two years in ponds in Indiana. Here the minnow seine method can be successful only about half the time at best!

However, it also appears that the minnow seine often does not even provide an accurate picture of the success of reproduction. This is substantiated by some data obtained by Messrs. Swingle and Wallace L. Anderson. On July 10, 1947, they visited several ponds in Indiana, checked them, and made remarks on the balance or imbalance of the fish populations which were summarized by Anderson (1948. Prog. Fish Cult., 10(3): 111–116). At the Curtis Shake farm in Knox County, several hauls with a minnow seine by Mr. Swingle and myself indicated, according to a copy of Mr. Anderson's field notes, that "Balance o.k. Bass reproducing this year, but too early for bluegills. An adequate number but not too many last year's bluegills were present." On August 20, 1947, some 6 weeks after the above check was made, we treated the pond on the Shake property with rotenone and recovered the following fish:

	Young of year	Others
Largemouth bass	130	15
Bluegills	0	6878
White crappies	1167	1912
Black bullheads	1758	13
Green sunfish	87	29

All of these fish must have been present and large enough to be caught in a seine 6 weeks earlier. Here the minnow seine technique was extremely misleading to say the least. Unfortunately, this is the only pond where a direct check with rotenone was made.

In another pond (Woodrow Boyer farm) checked the same day, (July 10) we found, according to Anderson's notes, that "Balance o.k. Both bass and bluegills reproducing this year." Checking some days later with a quarter-inch mesh, bag seine 30 feet long and 6 feet deep, we took 15 bass from 3 to 6 inches long; 150–200 young-of-the-year bluegills and 2 adults; and 100–150 young-of-the-year redear sunfish and 10 adults. The pond had been stocked in the autumn of 1945 with a mixture of redears, bluegills, and bass. Here, again, I can see no reason why the redears should have been missed on July 10 unless the gear used at the time was inadequate.

During the summer of 1949 we checked nearly 200 ponds in Indiana that had been

stocked experimentally. We used either a 30-foot or 50-foot bag seine with quarter-inch mesh for our sampling. On several occasions we sampled with a minnow seine as well but in every case we had to fall back on the data from the hauls with the larger seine because they included more species and a much wider range in sizes. Further, we obtained definite information on the growth rate and sizes reached by the parent fish. This method of sampling is not much more time-consuming than the one recommended by Swingle. The information gives a better overall picture and can be used without recourse to interpretation by hypotheses concerning the interactions of bass and bluegills, or other species of fish.

To summarize, we have found two glaring failures in the minnow seine method for determining balance in fish populations: (1) it very frequently does not reveal all the kinds of young fish present in the pond; and (2) even when this is accurate, the presence or absence of young fish does not necessarily indicate that the pond is out of balance.—LOUIS A. KRUMHOLZ, *Department of Zoology, Indiana University, Bloomington, Indiana.*

PARALDEHYDE AS AN AID TO HANDLING MAMMALS

Several methods are in current use for inactivating wild mammals so that they may be handled without injury. The mechanical devices such as holding boards (Fitzwater, 1943), wire cones (Erickson, 1947 and Aldous, 1946) and bags and nets are not completely satisfactory as they do not permit examination of dentition, detailed measurements or other examinations requiring complete inactivity of the animal. At various times in the past, ether and chloroform have been used as anaesthetics, but they are difficult to administer and may lead to respiratory diseases. More recently Howell (1937) has suggested the use of pentobarbitol sodium (nembutal) in the dosage of 1/6 grain per pound of animal weight given in food on an empty stomach, and Rausch (1947) the interperitoneal injection of the same drug in the dosage of one grain in one cc. of water per pound of animal weight. Shaw (1948) suggests the use of nembutal subcutaneously in dosages varying from 5 to 12 cc. for beaver. Nembutal is slow in action and requires several hours for complete recovery. Furthermore, interperitoneal injection is not readily accomplished with a struggling animal, and occasionally may be fatal.

An anaesthetic that could be administered intramuscularly with rapid action and that would wear off rapidly was desired for handling beaver. Paraldehyde ($C_6H_{12}O_3$), a polymer of acetaldehyde, was investigated. Goodman and Gilman (1941) describe its chemical and pharmaceutical properties as follows. It is a liquid with a pungent unpleasant odor and a burning taste. It is usually administered orally or rectally in man, but can be given intramuscularly or intravenously. Paraldehyde has a wide latitude of dosage, since in humans 15 times the usual dose has been given without ill effects. It is a sleep-inducing hypnotic, not a painkiller, acting principally on the central nervous system, and with no known after effects. Sufficient stimulation will awaken an animal under its influence. There is little danger of infection in wild mammals since it is self-sterilizing. Sterile water or physiological salt solution can be used as a dilutent, but the solubility is only 12 cc. per 100 cc. of water at 12° C. and decreases to about 5 cc. per 100 cc. of water at 100° C. Dilution is seldom necessary if a small tuberculin syringe is used. The freezing point is +10.5° C.

Proper dosages of paraldehyde induce sleep, or at least drowsiness to the point of easy handling, within 10 minutes. Like all drugs which are excreted rapidly, dosage depends more nearly upon metabolic body size (surface area) than on weight (Kleiber, 1947; Crawford, Terry and Rourke, 1947). The dosage in cc. for beaver may be calculated as: $0.45\ W^{2/3}$ where "W" is the weight in kilograms (beaver surface area in square meters conforms closely to the formula $0.1\ W^{2/3}$). No constant formula for surface area for other animals was found; dosages based on weight are suggested as follows:

Weight in Kilograms	Dose in cc. per Kilogram
0.020	0.8
0.250	0.52
0.700	0.4
3.5 to 5	0.3
10 to 12	0.25
70	0.1

These dosages are based on the authors' trials on 2 meadow mice (*Microtus pennsylvanicus*), 3 white-footed mice (*Peromyscus maniculatus*), 10 guinea pigs (*Cavia cobaya*), 1 gray squirrel (*Sciurus carolinensis*), 1 chipmunk (*Tamias striatus*), 2 raccoons (*Procyon lotor*) and 7 beaver (*Castor canadensis*). They may be used as a tentative guide, but any investigator will soon develop a more accurate dosage for the animals with which he is concerned, since species, sex, age, morphology and physiological condition may alter the dosage. It is best to start with a small dose. If the dose is too small, a slight additional amount may be injected to attain the desired results.

The case histories of two beaver will serve to illustrate the results which may be attained with proper dosages. The first aninal was a male weighing 4.2 kg. A dose of 1.3 cc. was administered in the upper muscle of the hind leg. After 5 minutes he could be handled readily; after 10 minutes he was asleep; at 23 minutes he was aroused by shaking, and walked to the water and swam away. This beaver was taken subsequently and showed no evidence of harmful effects. A second beaver male weighing 11.5 kg. was given 2.2 cc. in the upper muscle of the hind leg. The animal could not be handled readily after 15 minutes, so 0.7 cc. more was administered. After 6 minutes he could be readily handled; after 14 minutes he was asleep; and at 50 minutes he stood up and walked.

LITERATURE CITED

ALDOUS, S. E. 1946. Live trapping and eartagging of muskrats. Journ. Wildl. Mgt., **10**(1): 42–44.

CRAWFORD, J. D., M. E. TERRY, and G. M. ROURKE. 1949. A simplification of drug calculation by application of the surface area principle. In Press. (Read in abstract form at the Annual Meeting of the Society for Pediatric Research, Atlantic City, May 4).

ERICKSON, A. B. 1947. A multiple type rat and mouse holder. Journ. Wildl. Mgt., **11**(4): 351.

FITZWATER, W. D., JR. 1943. Color marking of mammals with special reference to squirrels. Journ. Wildl. Mgt., **7**(2): 190–192.

GOODMAN, L., and A. GILMAN. 1941. The pharmacological basis of therapeutics. New York: Macmillan Co., pp. xiii+1381.

HOWELL, A. B. 1937. Anaesthetizing mammals. Journ. Mammalogy, **18**(4): 513.

KLEIBER, M. 1947. Body size and metabolic rate. Physiol. Reviews, **27**(4): 511–541.

RAUSCH, R. 1947. Suggestions for the handling of certain mammals. Journ. Wildl. Mgt., **11**(2): 189.

SHAW, S. P. 1948. The beaver in Massachusetts. Research Bull. No. 11, Mass. Dept. of Conservation. Pp. 48.

—JOHN L. BUCKLEY, *Department of Forest Zoology, New York State College of Forestry, Syracuse 10, New York;* and WILLIAM H. BERGSTROM, MD, *Ass't. Resident in Pediatrics, Syracuse Memorial Hospital, Syracuse 10, New York.*

EFFECT OF INTRODUCTIONS ON TWO GAME POPULATIONS IN CZECHOSLOVAKIA

When Carl Ausserer of Austria published his book on the Steinbock or Ibex (*Capra ibex* L.) in 1947 (Der Alpensteinbock, Wein, Universum), European zoologists and hunters were astonished to learn how much this wild goat, strictly protected for many years, had suffered during the Second World War. According to Ausserer's book, there are in Europe hardly 2,000 Ibexes at the present time, the largest population of them (about 910–1030) living in the Swiss Alps, about 843 in Italy, and hardly 50 in Austria. Nothing is known of the destiny of Ibexes in the Bavarian Alps in Germany.

As Ausserer makes no mention of the present Ibexes in the High Tatra mountains of

Slovakia, the following notes will be of some interest to American game managers and mammalogists.

About the first half of the 19th century, the Ibex became extinct in the High Tatra (elevations up to 8,500 feet). In 1901 some Steinbocks were re-introduced from Austria by the late Prince Hohenlohe into his hunting areas in High Tatra. The origin of these animals was doubtful, since the exporter, Prince Pless, was well known for his interbreeding experiments on different species of the genus *Capra* and the Ibex in particular. Some years after the first introduction, two Asiatic species were unfortunately also introduced into these mountains: the Bezoar-goat (*Capra hircus*) from Asia Minor and the Sinaitic-goat (*Capra nubiana*). These two new species interbred with the Ibex, and the general aspect of that creature therefore changed: its general color and horns varied, and the beards of the males became longer.

The cross from this miscalculated introduction also produced physiological changes that were fatal to the Ibex stock. The true Ibex (*C. ibex*) breeds in mid-winter (December-January), and its young are born in April-May. The two species introduced from their warm Asiatic climate breed in late summer (August-September), their young being born in February-March. As a result of the interbreeding of *ibex* x *hircus* x *nubiana*, the breeding season of the Tatra Ibex shifted to late summer. Thus after a gestation period of about 5 months, the young were born in February-March at a time when in these mountains there are snow storms and temperatures far below 0° C. So the offspring perished from generation to generation. With protection, feeding, and management before World War II, Tatra "Ibexes" numbered more than 50. With illegal hunting and practically no care during wartime, there now survive only four female Ibex-like creatures as the last monuments of a once-powerful animal in the wild parts of the High Tatra mountains.

Still another unfortunate introduction took place before the first World War when some feudal lords introduced into Slovakia the Siberian race of the roebuck (*Capreolus capreolus pygargus*) which is much larger than the typical native race of the country. The two subspecies interbred at some localities. When a native, typical female interbred with a Siberian male, the large size of the foetus made parturition impossible, and the female perished. Such cases were observed especially in the northern parts of the country. At the present time, no similar records are available, but the effects of interbreeding are still perceptible when bucks with abnormally high and thick antlers are shot.

Thus, the experience from these two unfortunate introductions might be that intelligent game management involves not only money and will; a knowledge of zoology is needed too.—F. J. TURČEK, *State Institute for Game Research*, Banská Stiavnica, Czechoslovakia. (Transmitted via J. J. Hickey.)

Accepted for Publication May 10, 1950.

DETERMINATION OF WEASEL SEX RATIOS BY PELT EXAMINATION

While engaged in studies at the Waterfowl Research Station at Delta, Manitoba, during the summer of 1947, I met a farmer named Murray Werbiski who had trapped weasels in the region since 1939. He had taken more than 600 weasel pelts in 8 years and well deserved his reputation as an expert trapper. In addition he has always been an acute observer and thinker. He knew by common name the species he trapped and readily distinguished the sexes both by means of the penis bone and by the differences in the gonads themselves. He had also noted, while pelting his animals, the difference in skull proportions in the two sexes.

Mr. Werbiski has made the following important observation: in years when weasel populations are low only 20% of his catch were females; in years when the population was increasing 40% of his catch were females. Because he has been an intensive trapper, working one section of marsh until the last weasel was taken—as indicated by tracks in the snow—his observation of the sex ratios obtaining during different population levels, is probably very reliable. This suggests that a compensatory means of population adjustment may

exist by which a low density in some way results in a greater proportion of females, thus increasing the fecundity of the species.

This unusual weasel trapper also pointed out that each sex of each species of weasel usually fell in a distinct fur grade. This immediately suggested that, if true, examination of fur buyers's records would provide us with much illuminating information concerning the ups and downs in the population of at least one important fur group.

To investigate this possibility I visited the warehouse of Soudacks' Fur Auction Sales, Ltd. in Winnipeg, Manitoba, and was permitted to examine 9,061 weasel skins on hand just prior to the August auction.

Determination of the species when pelts alone are available must be based primarily upon comparative length and diameter of tail as compared with the rest of the pelt. The much longer tails of the long-tailed weasel (*Mustela frenata*) as compared with those of ermines (*M. erminea*) permitted easy separation of these two species. Ermines were distinguished from least weasels (*M. rixosa*) by the larger diameter of the tail, it being nearly twice as great as viewed in the dried pelt. Because of the diverse methods of pelt preparation used by various trappers, absolute sizes and measurements were of little value. Neither was the amount of black on the tip of the tail found to be very helpful. Although it was never extensive in least weasels, much variation was noted in each of the three species and some pelts from all three were examined that showed absolutely no black at the tip of the tail. When the pelts had been reversed in drying, the tail tip often had been incompletely turned and so upon superficial examination appeared to be without a black tip. Upon closer examination these truncated tips could be readily distinguished from those tails which were complete but devoid of any black.

LEAST WEASELS

Of the 9,061 weasel pelts only 168, or 1.7%, were found to be those of *M. rixosa*. All strings of weasel furs that might contain those of least weasels were carefully checked for their presence; these included strings marked *kits*, *small*, *mixed lots*, *damaged*, and *tainted*.

SEX RATIOS

It was found that the sex could be readily distinguished on most pelts. Unless the skins were badly damaged or severely trimmed the preputial orifice or some other remnant of the penis, occasionally including the os baculum, was obvious toward the posterior end of the ventral side. Absence of this mark indicated a female. No trace of nipples was ever found on female pelts.

This criterion of sex in pelts seems to be reliable for all mustelids and provides a convenient tool for readily classifying the sex composition of the fur harvest.

FUR GRADES

Although all the 9,061 weasel pelts on hand could not be individually checked during the one day the fur was available for examination prior to the auction, several strings of each of the larger sizes, in addition to all the strings containing smaller furs as mentioned above, were actually examined one by one—2,614 being studied in this manner. From this study the following conclusions were drawn.

Fur Grade	
XX Large	= all *M. frenata* of which 6/7 are males.
X Large	= all *M. frenata* of which 7/8 are females.
Large	= either *M. frenata* females or *M. erminea* of both sexes.
Medium	= all *M. erminea* of which 2/3 are males.
Small	= all *M. erminea* of which 19/20 are females.
Kits	= either *M. erminea* mostly females (75/77) and *M. rixosa* of both sexes.

It is clear from the above table that nothing can be learned from fur records concerning the percentage of female least weasels that occur in the harvest from year to year, but for the other two species it seems very probable that any major shift in the proportion of the sexes could be detected. If the percentage of *Smalls* in the fur harvest rose it would indicate an increasing proportion of females among the ermines. Similarly if the percentage of X *Large* as compared with the XX *Large* increased, an

increase in the proportion of females among the long-tailed weasels would be evident.

If fur records are to be used to detect this supposed inner compensatory population adjustment, sex ratio shifts of the apparent magnitude of 10–20% must be detectable by examination of fur records. The error in determining actual sex ratios from fur records as based on the above table are indicated below for a hypothetical population of *M. frenata* showing a fluctuation of 20% in sex composition over a three-year period.

Year	Anticipated Actual Sex Ratio		Probable Fur Grade Classification	
	Males	Females	Males	Females
1	80	20	71	29
2	70	30	64	36
3	60	40	56	44

The probable fur grade classification for any one year was computed by subtracting the 1/7 males that would be incorrectly classified and adding this fraction to the female column, and by subtracting the 1/8 females that would be incorrectly classified and adding this fraction to the male column. Differences of the magnitude of 10% should, therefore, be readily detected by fur records when several thousand pelts of one species have been purchased by a fur house where grading probably was consistent from year to year.

This technique should make possible analysis of fur house records for shifts in sex ratios of either ermines or long-tailed weasels and correlation of these shifts with trends in total harvest. Perhaps this technique may be used only on a local scale because populations of weasels may not vary together over large regions and sex ratio shifts in one area may be opposite to those in an adjacent area, thus cancelling out in the fur house records.—WILLIAM H. ELDER, *Missouri Cooperative Wildlife Research Unit, University of Missouri, Columbia.*

Accepted for Publication June 13, 1950.

NOTES ON AGE DETERMINATION IN JUVENAL EUROPEAN QUAIL

Almost 40 years ago, Louis Bureau (1911) in France studied the replacement of the juvenal plumage in the Hungarian partridge (*Perdix perdix*) and (1913) red quail (*Alectoris rufa*). He provided detailed information apparently permitting the rather accurate determination of age in first-year specimens of these species.

Some of his findings in this connection have been given in earlier papers (Petrides **1942**: 320–321, Petrides and Nestler, **1943**: 774). McCabe and Hawkins (**1946**: 43), however, found Bureau's papers to be "virtually inaccessible."

While the only source of the original publication known to me is the U. S. Department of Agriculture Library, Washington, D. C., photographs of Bureau's tables are still in my files. In view of an increasing number of population studies being undertaken by means of age ratios, it seems possible that Bureau's findings on these species might prove to be of general interest.

While his data on Hungarian partridges may at once be useful in working with North American populations, his information on red quail may not be so directly applicable. The latter species is closely related to the Chukar partridge (*A. graeca*), however, which is now well established, at least in Nevada. His red quail figures, therefore, may be of use there and elsewhere pending more detailed studies of the Chukar.

Tables 1 and 2 are adaptations of Bureau's age determination tabulations which are more detailed but probably not much more useful than these summaries. It will be recalled that the primary wing feathers are numbered by counting outward toward the wingtip. The short axial secondary feather serves as a convenient starting point and can be counted as zero. The outer two juvenal primaries (numbers 9 and 10) are retained throughout the birds' first year. Their pointed shapes, and that of their greater coverts in the Hungarian partridge, assist in identifying birds of the year.

The approximate ages at which McCabe and Hawkins (**1946**: 44) diagrammed the shedding of the juvenal primaries on their Hungarian partridges also is given in Table 1.

TABLE 1.—AGES OF HUNGARIAN PARTRIDGES IN DAYS AT VARIOUS STAGES OF POST-JUVENAL MOLT (ACCORDING TO BUREAU, 1911)

Primary Number:	1	2	3	4	5	6	7	8
Dropped, 0 mm. long	24	27	33	39	47	55	67	86
1/4 grown, 30 mm. long	—	33	39	45	53	61	74	94
1/2 grown, 57 mm. long	—	—	—	—	—	66	81	101
3/4 grown, 83 mm. long	—	—	—	—	—	—	86	108
Fully grown, 110 mm. long	—	—	—	—	—	—	—	116
Feathers dropped, according to McCabe and Hawkins, 1946:	24	31	38	45	52	59	73	87

TABLE 2.—AGE OF RED QUAIL IN DAYS AT VARIOUS STAGES OF POST-JUVENAL MOLT (ACCORDING TO BUREAU, 1913)

Primary Number:	1	2	3	4	5	6	7	8
Dropped, 0 mm. long	29	34	41	49	58	70	86	105
1/4 grown, 32 mm. long	—	40	48	56	65	77	93	112
1/2 grown, 60 mm. long	—	—	—	—	—	84	100	118
3/4 grown, 88 mm. long	—	—	—	—	—	—	106	124
Fully grown, 116 mm. long	—	—	—	—	—	—	—	130

LITERATURE CITED

BUREAU, LOUIS. 1911. L'age des Perdrix: I. La Perdrix grise. Bull. Soc. Sci. Nat. Ouest France (Nantes), ser. 3, **1**(1–2): 124 pp. 35 figs.

———. 1913. L'age des Perdrix: II. La Perdrix rouge. Bull. Soc. Sci. Nat. Ouest France (Nantes), ser. 3, **3**(2): 143 pp. 53 figs.

MCCABE, ROBERT A. and ARTHUR S. HAWKINS. 1946. The Hungarian partridge in Wisconsin. Amer. Midland Nat. **36**(1): 1–75.

PETRIDES, GEORGE A. 1942. Age determination in gallinaceous game birds. Trans. North. Am. Wildlife Conf. **7**: 308–328.

PETRIDES, GEORGE A. and RALPH NESTLER. 1943. Age determination in juvenal bobwhite quail. Amer. Midland Nat. **30**(3): 774–782.

—GEORGE A. PETRIDES, *Division of Conservation, Michigan State College, East Lansing, Mich.*

Accepted for Publication May 30, 1950.

THE USE OF SCENT IN LIVE-TRAPPING MUSKRATS[1]

While trappers have for many years successfully used a scent, or lure, to attract muskrats to their traps, no published account of its use by wildlife technicians in research or management projects has been found, although live-trapping has been involved in many recent investigations of this furbearer (Aldous 1946, Hensley and Twining 1946, Shanks 1948, Takos 1943).

The results of a live-trapping and tagging program carried out as part of a population study of muskrats at Gray's Lake, Idaho pointed out the effectiveness of scent as an attractant and suggested that it could be profitably used as supplementary technique in this type of work.

During the summer and early fall of 1949, 170 muskrats were live-trapped in 522 trap nights. For the first half of this period carrots were used as bait in the National live-trap. A prepared muskrat lure was used in combination with the carrots during the final phase of the study. The difference in the degree of success resulting from the two different methods is summarized in Table 1.

[1] Contribution from the Idaho Cooperative Wildlife Research Unit, Moscow, Idaho; Idaho Fish and Game Department, University of Idaho, Wildlife Management Institute, and U. S. Fish and Wildlife Service, cooperating.

TABLE 1.—TRAPPING SUCCESS ATTAINED WITH DIFFERENT ATTRACTANTS IN LIVE-TRAPPING MUSKRATS

Attractant Used	Total Number Trap Nights	Total Number of Successful Traps	Average Trapping Success
Carrots only	279	66	23.6%
Carrots and scent	243	104	42.8%
All methods	522	170	32.6%

All trapping was confined to the same general area and conditions of weather and water levels were similar throughout the entire period. Fifteen to twenty traps were used but were not set every night.

Since the two types of attractant were not used concurrently, the possibility that some factor other than the scent was responsible for the increase in trapping success must be considered. Except for the weather however, which was constant at the time, such a factor could hardly account for the abrupt increase that followed introduction of the scent to the sets.

During the six days just preceding use of this lure, trapping success for individual nights ranged from 14 to 33 per cent and averaged 25 per cent. For the first six nights the scent was used trapping success ranged from 22 to 72 per cent and averaged 45 per cent.

Commercially made scents of this type are thick, pasty liquids containing a mixture of ingredients designed to produce an odor that attracts the muskrats through an appeal to their mating instinct. These lures are readily obtainable from the many well advertised trappers' supply houses and are usually sold for $1.00 per ounce.—ROGER M. WILLIAMS, *Idaho Cooperative Wildlife Research Unit, University of Idaho, Moscow, Idaho.*

LITERATURE CITED

ALDOUS, SHALER E. 1946. Live trapping and tagging muskrats. Journal Wildlife Management, 10(1): 42–44.

HENSLEY, ARTHUR L. and HOWARD TWINING. 1946. Some early summer observations on muskrats in a northeastern California marsh. California Fish and Game, 32(4): 171–181.

SHANKS, CHARLES E. 1948. The pelt-primeness method of aging muskrats. American Midl. Nat., 39(1): 179–187.

TAKOS, MICHAEL J. 1943. Trapping and banding muskrats. Journal Wildlife Management, 7(4): 400–407.

REVIEWS

Conservation of Natural Resources, edited by Guy-Harold Smith. John Wiley and Sons, Inc., New York, N. Y., xii+552 pp., illus.; 1950. $6.00.

This book is a revision of "Our Natural Resources and Their Conservation" by Parkins and Whitaker, published in 1936. It has been largely rewritten; several new contributors have been added, and some of the original authors dropped. It is a college textbook for the general student, composed of 23 chapters by 20 authors and edited by one of them. Most of the authors are from mid-western colleges.

The editing has been light and it suffers from the usual troubles of multi-authored texts: duplication of material, variable style, differing viewpoints, and lack of coordination. It does not delve far enough into resources problems to offer much stimulation to the professional conservationist.

The topics are treated broadly and, to a considerable extent, from the geographer's point of view. As an orientation text, it covers the field satisfactorily and as well as any available.

Opening brief chapters cover the history of conservation and the public domain in America. Then follow seven chapters on agricultural resources, two on forestry, four covering water resources, two on minerals, two on wildlife and fisheries, and two on man. Lastly, there are two chapters on planning—local, state and national.

There are notable instances where the authors seem not to be fully intimate with their subject; for example, in developing the history of conservation, the Soil Conservation Service and the Production and Marketing Administration functions of the U. S. Department of Agriculture are confused. The demonstration project era of soil conservation work of the '30s is covered in some detail but the vitally more significant development of soil conservation districts work since 1937 is not mentioned. On the other hand, a relatively unimportant post World War II land rush for a few thousand newly-irrigated acres in the West is given prominence.

The chapter called, "Tree Crops", substantially the same as in the earlier book, seems somewhat visionary. It starts out by saying, ". . . it is time that we developed a *man's* agriculture . . ." Since ancient woman domesticated wheat, corn, cotton, etc., in this ". . . age of science . . . now arrived, . . . the time has come to develop a man's agriculture, new crops —crops that grow on trees." Some of those discussed as possibilities are: keawe, carob, honey locust, chestnut, cork oak.

Of particular interest to wildlifers are the chapters on wildlife by Charles A. Dambach, and on fisheries by Howard H. Martin. Dambach's presentation is refreshingly ecological in its treatment. The phases of wildlife conservation—history, economics, populations, environment, management problems and methods of solution—are well-balanced. The controversial questions of predator control, refuges, restocking, game ownership, pollution and hunting pressures are handled with good sound sense. The place of various public and private agencies in wildlife programs is summarized simply and clearly.

The chapter, "Fisheries and the Future" is concerned entirely with commercial fisheries except for a single paragraph on farm ponds. It is largely devoted to marine problems with the most attention given to Pacific Ocean species.—FRANK EDMINSTER.

California's Fish and Game Program. Revised May 5, 1950. Report to the Wildlife Conservation Board, Seth Gordon, Consultant. Published by the Senate of the State of California. 245 pp.

The California Wildlife Conservation Act of 1947, which created a Wildlife Conservation Board in the Department of Natural Resources, which provided funds and which set forth certain objectives, embarked the state upon an entirely new venture. The Act not only sets an enviable pattern in itself, especially in the generous financial support provided, but in its provisions for sound approach to the problems. The board named under the Act has further established an enviable record in the plans it made to further the purposes of the Act and toward a sound analysis and recommendation for procedures.

The report of the board is, in our opinion, a reading "must" for administrator, technician and layman. Throughout, it is an outstanding presentation. In fact, it is so much more than a report that it should not be thought of in the usual sense of that term but rather with the emphasis used on the cover page; i.e., "A Fish and Game Program".

Upon opening the cover, one's interest is immediately aroused by four attractive color plates—the state flag, the state bird, the state fish and the state flower. You want to see more. A brief, clearly-stated "Foreword" gives the background; one isn't discouraged trying to find out what it is all about. The placing of the "Table of Contents" just after the foreword is especially good, because one can absorb at a glance the general plan and get some idea of where to find what he is looking for. Again, the general summary contained on pages 9 to 12 and the chapter summaries preceding each chapter are especially commendable. Chances are that because of these brief, bird's-eye views many more who should read it will do so. They give the report an aspect of frankness and aid the reader to actual understanding. Would that more such efforts intended for actual working use were provided with general and chapter summaries such as incorporated in this one! We have read and tried to use many reports intended for program work and have usually floundered around in a wilderness before ever unravelling the objectives or recommendations. This presentation is a valuable departure deserving high commendation. Because of the complete and condensed picture so drawn, we predict the report will be used and understood and will be effective.

While many other functions were assigned to

the Wildlife Conservation Board, time will probably prove the document prepared under the able leadership of Seth Gordon to be one of its most important accomplishments. The method of gathering and presenting data, the format, the skillful combining of investigational results, recommendations, fundamental wildlife management principles, and actual first steps of program accomplishment is a pattern that may well serve as a guide to many states. Not only are data presented, but sources of calculation are set forth in such a way that credence is added to statements and recommendations even in the minds of the doubting and uninitiated. It is not only a report of survey but is a program and a progress report of work done. The table of project allotments already made by the board should be impressive to even the most skeptical.

We rate this report tops in public relations value because:

1. Presentation of data clearly shows public participation in the surveys and gives the public confidence that their ideas were given consideration.
2. There is every evidence that final recommendations grew out of needs as found and were not a compilation of preconceived ideologies. There are few points which would justify any criticism that sound management has been side-tracked.
3. It is put together in such form that the uninitiated can absorb the program step at a time and go into management as deeply as they can probably understand. A person can find what he wants without great difficulty or long discouraging reading. In fact, the manner of presentation is such as to *encourage* careful perusal on the part of the public and result in high educational values. You read the chapter summary *first* and then *want* to read more.
4. The manner of presentation is such that it furnishes a vehicle through which a heterogeneous mass of population can tie together their own thinking about such a program, because they can find so much that starts from their own ideas.
5. Confidence is inspired by a consideration of the calibre of the technicians and advisors called in to participate in the investigations and formulation of recommendations. And, whether with or without intent, the somewhat casual manner of presenting the qualifications of these men in connection with the context adds weight to the public relations value.

In approaching an evaluation of the report on a more detailed basis, we find ourselves confronted with several major perplexities. The weaving together of so much boiled-down investigation, recommendation, program and educational material poses immediately the problem of what the proper basis is for judging it. One finds himself tempted to enter into discussion and discourse much beyond what any review should attempt.

From the viewpoint of the purely technical-minded there could be some criticism of contradiction. For example, there are contained throughout all of the management parts excellent discussions of fish, birds and upland game, emphasizing the value of environmental and habitat management. At the same time there is recommended a large program of fish hatcheries and game farms. There is every reason to believe, however, that the intent of the program is to push toward habitat control and away from the game and fish farm idea as an ultimate solution. Chapter VIII, "State Game Farms—Conclusions and Recommendations", is a presentation of a bit of history and experience in the field of artificial rearing both by states and laymen, that should be read by everyone interested in perpetuation of our wild species. The nation-wide headaches, the white-elephant sportmen's projects that have squandered public and private funds, the bulldog attitude that more game farms and more fish hatcheries are the panacea for game ills are exposed to pretty glaring lights in this chapter.

The report is particularly strong in its constant awareness of the impact of man and his operations upon the wildlife management field. This includes many very forthright statements regarding farmer-sportsmen relationships.

The whole of the big game section, probably a bit more than any others, is a text book on game management. We are particularly impressed with the burden of thought that management must shoot at optimum populations as related to environment rather than at a crowding in of a maximum population which

becomes its own downfall, that surplus deer crops must be *harvested* in order to cope with the damage and starvation problems, that it is useless to spend money to increase deer range and browse when the present crop is not being adequately harvested, that such tools as controlled burning and refuges have their limitations. Although time would probably not have permitted, one regrets that the problem of the big-horn sheep was not as thoroughly gone into as some of the other problems.

As regards waterfowl, the report presents many angles that may be difficult of understanding to many people. It is well pointed out that the need for multiple water use in California is a very different thing than in many of the other states. The problems created by irrigation, for example, set up a whole new set of wildlife management problems. It is evident that California also has the problem of private *vs* public hunting opportunity. Not much is recommended as a solution for this except some purchase of lands for public use, but, at the same time, most of the states will gasp at the allocation of something over $4,000,000 to this part of the program. We think the "advisory board" plan as set up for handling some of the waterfowl program is a bit dangerous. How long will it be before this sort of board does for California what similar boards have done elsewhere; i.e., put waterfowl management into the hands of the shooter almost entirely? Also, we think the proposal made in the program recommendation for a system of "feeding" is a step backward. Admittedly, feeding in itself might help the crop damage problem, but the proposal as made smacks too much of a "dodge" to accomplish more duck killing. One cannot help but wonder how much of this part of the report was necessary as a "pacifier" for duck shooting clubs. We note that in one paragraph on page 169 it is made clear that the views expressed in this regard are a summary of composite thinking and that "no claim to originality is made by the writers." Is this a "hedge"? We also believe that the statement on page 164 to the effect that a food planting program for waterfowl is new to the state of California implies more credit for originality than is deserved. There are a number of places where such programs have been systematically carried on for some years.

There are expressed certain ideas about organization, legislation and fundamental administration that can easily be highly controversial.

In the final analysis no doubt we shall have to evaluate the whole of the recommendations and program for what they are intended to be; not a scientific treatise, but an analysis of problems and a recommendation for solutions that move forward, even though not as completely as some would wish, and with enough educational material woven in to give substance and acceptance by those who will in the long run have to effectuate and live under the program established. From this viewpoint it is perhaps understandable that many compromises were necessary to avoid getting so far out in front of the people that any adoption would become impossible. If this is so, one can understand that probably it was necessary in many instances to propose a fifty-fifty solution, emphasizing ideals and fundamentals while at the same time suggesting things that sound contradictory. A fair evaluation must no doubt be grounded upon an understanding of the nature of the state's population and a constant reminder of the fact that the job in hand was a study of present situations and local conditions and the development of a program that could be made to work in California. A very large part of the state's population is not a "native population" as we are apt to think of it. The people are new, or are semi-transient, or have had their youth-roots in another part of the country. Their feeling toward native things may be very different than in other states, and their sense of long-time values may be much more ephemeral than most of us are accustomed to. The key to the nature and objectives sought probably lies in a statement in the last chapter: "It is hoped the report will be widely read and discussed, and that a broader understanding of the future requirements of fish and game may result therefrom." It's purpose was to draw a pattern for program procedure, not to set up a Utopian state. From this standpoint we feel it is a job admirably done.—I. T. BODE, *Director, Missouri Conservation Commission, Jefferson City.*

Proceedings and Papers—International Technical Conference on the Protection of Nature—Lake Success, August 22–29, 1949. Edited by

The International Union for the Protection of Nature. Published jointly by Unesco and The International Union for the Protection of Nature, 42 rue Montoyer, Brussels, Belgium, 1950; pp. 1–583. $4.00.

The Conference covered in these Proceedings was jointly sponsored by the newly formed International Union for the Protection of Nature and Unesco. In all, some thirty-two nations and colonies were represented either by an official delegate or an observer. Most of the papers and the discussions dealt with either educational or ecological problems.

One panel under the chairmanship of William Vogt devoted three days to a thorough discussion of the ways and means of bringing about a greater public appreciation throughout the world of the need for nature protection. The papers presented left little doubt as to the urgency of the problem, but were rather discouraging as to the degree of progress to date. Even in the United States, where there are many more media for reaching the public than in most countries, we have made only a beginning, and in Africa and Asia the idea is almost a wholely new one.

Five days were devoted to the broad problem of international co-operation in the stimulation of ecological research in various fields of the natural sciences, a day each being allotted to the following five topics:

a) The effects of "planned enterprises" on the balance of nature.
b) Possible consequences on nature's equilibrium of the generalized use of pesticides (DDT, 2,4D, etc.)
c) Possible consequences on nature's equilibrium of the generalized use of "Antrycide" (against the sleeping-sickness of cattle).
d) The consequences of uncontrolled introduction of exotic species on the balance of nature.
e) The problem of vanishing big game herds in parts of the world, e.g. in Asia and in Africa. The interrelated forces, in new countries, of advancing civilization, native population and wild fauna to be studied and action recommended.
f) Emergency action for preserving vanishing species of flora and fauna.

In general, the problems are well stated, but unfortunately the papers and the discussion seem weak on practical suggestions concerning what can be done to correct the admittedly bad conditions that now exist. The session devoted to the last topic (f) led to the drawing up of a list of twelve birds and fifteen mammals that are at present in a seriously reduced state and in need of emergency action if they are not to become extinct.

Several proposals for action came before the Conference. One called for the establishment by the I.U.P.N. of an "International Survival Office" at its headquarters in Brussels for the purpose of maintaining a continuous check on threatened species. Another proposal concerned the calling of a World Convention on the Protection of Nature patterned after the 1933 London Convention for the Protection of African Fauna and Flora and the 1940 Convention on Nature Protection and Wildlife Preservation in the Western Hemisphere. A critical discussion of these proposals brought a vote of approval for the first and a postponement of any immediate consideration of the second.

These Proceedings are interesting reading for anyone who has a broad interest in nature protection, as almost every aspect of the problem is at least touched upon and the present nature protection situation in most parts of the world is well covered. The Proceedings are in both French and English, but the papers (generally condensations of the original) are in a single language, either French or English.—RICHARD H. POUGH.

Michigan Deer, by I. H. Bartlett; Game Division, Department of Conservation, Lansing, Michigan; June 1950, pp. 50, illus. plates and figures.

This latest bulletin on Michigan's deer problem is a successor to "Whitetails," by the same author, published in 1938.

In the 12-year meantime, developments have been according to expectation. The deer herd goes into each fall numbering about a million animals, agricultural damage has become "a major problem," and winter starvation areas regularly add their annular rings of growth around the foci that had become evident by the mid-thirties. What probably was not expected is the unfortunate circumstance that,

as of 1950, Michigan still has no legal basis for managing what may be the largest state deer herd in the nation.

The bulletin is organized around three chapters embodying information on History, Recent Conditions, and Management. Each chapter gives separate treatment to three major divisions of the state, the Upper Peninsula, Northern Lower Peninsula, and Southern Lower Peninsula.

Of the latter region, 90 percent is agricultural or otherwise unsuitable as deer range. The herd is small and the principal issue is crop damage. Damage complaints prompted the legislature to open Southern Michigan to buck hunting with shotguns in 1948 and this policy continues.

The state's principal deer range is in the two northern regions. In the Upper Peninsula, 298 yarding areas have been field surveyed, which is estimated to be a coverage of 80 to 90 percent. Since only 10 percent of the land is suitable for wintering, the bulk of the deer herd is concentrated in these tracts for 3 to 4 months annually. Approximately one-sixth of the Upper Peninsula yarding area has a plentiful food supply; on about half of the winter range deer still find enough to eat; the remaining third provides insufficient browse and starvation is a prevailing threat, although losses have not been high in the past few years. In 1949 the reported success of UP hunters was 49 percent, which represents a kill of 2.5 bucks per square mile. Drive counts, the last of which were taken in 1940, indicated summer and fall populations varying from 10 to more than 30 deer per square mile. The present kill is roughly 40,000 bucks from a herd of 400,000 animals.

Approximately 63 percent of the Northern Lower Peninsula is deer range and the remainder is in agricultural use. A total of 556 yarding areas have been inspected, and conifer swamps suitable for wintering are estimated to comprise 7 to 8 percent of the land. Summer populations of 40 to 50 deer per square mile are forced into these yards in hard winters. The state's most critical starvation areas are in the north central portion of this region, where overbrowsing and heavy winter losses have been increasing since 1930. In 1936 deer from 500 square miles of range wintered in browsed-out yards and ". . . starved by thousands. Similar winter food shortages have spread until they now occur in yards supporting deer from 4,000 square miles of range and starvation losses are estimated to be as high as 50,000 animals during severe winters. . . ."

It is believed that recent winter losses have been offset by herd increases in semi-agricultural range and that deer in this region total about 600,000. The computed kill in 1949 was 66,669 bucks. The illegal kill of does and fawns has been estimated at from 10,000 to 50,000. As range deteriorates, a decline of the herd appears inevitable, and a portent of this is seen in a recent reduction in the annual fawn crop. November counts indicated 80 fawns per 100 does in 1945 and 51 in 1949. "This trend will be important if it continues downward."

Michigan has run the usual gamut of winter feeding experiments, even though state biologists saw 20 years ago that this means of temporarily holding too many deer in a few accessible areas could only aggravate the situation.

The buck law was passed in 1921 and, in the main, has been held sacrosanct since then. Exceptions have involved the harvest of an insignificant number of antlerless deer. Bow-and-arrow regulations have been liberalized steadily since 1937 and in 1949 there were 66 counties in which "any deer" could be so taken. In 1941, a law was passed providing for a deer of either sex for camp use, but its repeal followed reports "that the deer herd had been ruined." Also in 1941, as a result of farmer-pressure growing out of crop damage, the legislature gave the Conservation Department authority to permit the killing of antlerless deer in Allegan County. In 1949, for the same reason, this authority was extended to nearly all of Southern Michigan and to seven counties in the fruit growing area along Lake Michigan farther north. The legislature has steadfastly refused to delegate authority to the Department under which regulations for the state as a whole could be liberalized and local herds managed in terms of winter-range carrying capacity.

This bulletin is a well-worded and cautious suggestion by a technical agency that it be authorized to do the job that might logically be expected of it. Michigan's deer specialists have reached the same conclusions as those of many another state. They know that a reasonably healthy herd of a million deer will renew

itself by 40 percent or more each year and that annual losses must equal this. The state has an available hunter harvest approaching or exceeding a quarter of a million bucks, does, and fawns, yet sportsmen now are harvesting less than half this number of bucks and fewer than 5,000 antlerless deer.

It can be suspected, hopefully, that this waste is likely to be of increasing interest to Michigan sportsmen, for they are the group most patently concerned. A half or more of their deer harvest is rotting in the woods and the range from which future harvests must come is suffering what may well be permanent damage.

By definition, legislatures are incurious on technical matters, but they are finely attuned to superficial breezes of public opinion. In a small way the farmers have breached the wall. The state's technical staff have done all that could be expected of them. A realistic deer program now is waiting for an understanding between aroused sportsmen and their representatives.—DURWARD L. ALLEN.

Handbook of Freshwater Fishery Biology, by Kenneth D. Carlander. Wm. C. Brown Company, Dubuque, Iowa. 1950. 281 pp. 5 charts, figs. 1–2. –4.50. Reproduced by offset.

Fishery biologists interested in freshwater species will be delighted with this compilation by Dr. Carlander who has brought together many widely scattered data on life histories and populations of fishes. Unlike Karl F. Lagler's "Studies in Freshwater Biology" which treats largely with methods of study of fishery problems, the present work deals with the results reported in 1122 papers including those published during 1949. Also included is a considerable amount of hitherto unpublished information from theses, and from the files of the Fishery Research Unit, Minnesota Department of Conservation and the Iowa Cooperative Fishery Research Unit at Ames. Many valuable data are buried in the files of other conservation agencies and university libraries. This book points out the need for publication of such data and may encourage the dissemination of the information through inclusion in future editions of this work or by mimeographed additions that Dr. Carlander hopes to make available.

The coverage includes freshwater fishes of the United States and Canada as well as anadromous fishes that have at one time been established in freshwater. However, the Pacific Salmons, *Oncorhynchus*, are not included. The data are given in table form by species arranged in phylogenetic order. The index is adequate.

The information dealing with growth rates is most complete and fills 201 pages. Also included are conversion factors for transfers involving standard length, total length and fork length; weight-length data, condition index data; size at capture by age groups; calculated lengths; and egg counts. An additional section on population data gives creel census results in catch per hour, in various kinds of water and with different types of gear, as well as catch per trip, and annual yields. However, no effort was made to cover these subjects as fully as that of growth rates. Tables for converting measurements of length and weight, as well as factors for calculating condition index, are also included. The appendix contains charts by Phenicie and Bishop for the convenient calculation of condition index or factor and a chart by Carlander and Smith that may be used to calculate the growth rate of fishes from scale measurements. Included in the introduction is a short discussion of the method of preparing scales for study.

Perhaps this work will have its greatest usefulness as a guide to the literature dealing with growth studies since fishery workers and students will usually wish to go into greater detail than could possibly be included in such a tabular treatment. It will also serve as a ready reference for those who need information in a hurry for rough comparative purposes. In future editions it is suggested that all subspecies be treated separately, especially when there may be a difference in growth rate (as in *Boleosoma nigrum*). Some data on growth included in taxonomic papers have been overlooked. Typographical errors are relatively few except in the case of misspellings of scientific names in the bibliography.—E. C. RANEY.

Man on the Landscape / The Fundamentals of Plant Conservation. By Vernon Gill Carter. National Wildlife Federation, Washington, D. C. 129 pp. illus. 1949. $1.50.

This small book is a timely, and on the whole a successful attempt to redress the distortion of emphasis which has led conservationists to think first of wildlife, then forests, soils, and water, and only recently of vegetation. Mr. Carter believes that vegetation is even more important than botanists themselves have claimed, and he does a good job of demonstrating why this is so.

His chapters, "How do we live and grow?" and "The problem of maintaining (*or substituting for*) the climax," are especially interesting. So, also, are two appendices, in the first of which he properly insists that a "mastery of the fundamentals of plant conservation must precede and is prerequisite to an understanding of special problems" usually discussed as conservation; and in the other, gives valuable hints to teachers for demonstrating in the laboratory, the classroom, or the field, the significance of plants in keeping the landscape productive.

Because primers of this type are so much needed in the growing conservation education movement, and because Mr. Carter demonstrates originality and epigrammatic skill in expressing many land use concepts, certain inconsistencies and shortcomings must be pointed out. This, with the hope that some future, enlarged edition may prove even more satisfactory.

The principal inconsistency is the repetition of some of the early, uncritical estimates of soil damage, and some uncritical impressions of forest losses. This is not to deny serious soil abuse and widespread neglect of productivity, but the crucial question in soil erosion would seem to be not how many inches of topsoil were lost, but whether enough remain on a given site to allow economic conservation practices now and in the future. Consider the recovery of the dustbowls when the rains came. Likewise it is too easy to exaggerate timber losses by saying *tree* when we should say *saw timber*.

As in almost all conservation writing, there is a nearly complete disregard of the principle that a rising economic margin resulting from technological progress is more often the cause of agricultural displacement than the land abuse so universally pointed to. Indeed, it is time we gave careful consideration to the agricultural economist's belief that land abuse is often a symptom of competitive disadvantages, and the result, not the cause of economic distress.

Technically, the distinction between primary and secondary succession (pp. 75–6) seems confused, and the statement that "most of the originally forested area needs lime (and other fertilizers) *because* (italics mine) the woody vegetation itself is evidence of lower fundamental fertility than the grasslands" (p. 17) needs documentation. Nor does it seem that war is proving itself a control on human population! The numerous photographs are well selected but not so well printed.

We can all agree with Mr. Carter that our widespread acceptance of so much that is abnormal in our landscape as normal, is itself a sign of abnormality, and that the difficult task of science and conservation education is to overcome the incubus of this statistical norm. —ROLAND C. CLEMENT.

Gamonia: or, The Art of Preserving Game; and an Improved Method of Making Plantations and Covers, by Lawrence Rawstorne, Esq. with fifteen coloured drawings by T. J. Rawlins, taken on the spot; A New Edition with an Introduction and a Note on the Author's Game Register by Eric Parker, Shooting Editor of *The Field;* 1929. 256 pages; Herbert Jenkins Limited, 3 York Street, St. James's, London, S.W.1.

Dr. C. H. D. Clarke of the Department of Lands and Forests, Ontario, Canada loaned the book, "Gamonia," to Dr. Logan J. Bennett who in turn kindly permitted me to read it. The book so impressed me that I thought many members of the Wildlife Society would also find of interest a review of "Gamonia" with passages dealing particularly with planting methods and observations on pheasants quoted at some length.

The book was originally published for Lawrence Rawstorne by Rudolph Ackerman in 1837. Only a few copies were printed.

As indicated above, a new edition, the one reviewed here, was published in 1929 with an introduction and a note on the author's game register by Eric Parker.

The latter stated in his Introduction, page 6, "It is the first treatise devoted solely to the subject of pheasant rearing, covert shooting,

and the management of woodlands for the special purpose of preserving game." When printed, pheasants were being reared by hand in large numbers for shooting. Young pheasants were turned into coverts and beaten out later in "battues."

Mr. Parker believes that "Gamonia" probably owes its name to the fact that some ten years before its publication Sir Humphry Davy, a chemist, had published his book "Salmonia, or Days of Fly-fishing."

In his preface, page 54, Mr. Rawstorne states: "Trees have been put into the ground without proper preparation of the soil, without draining and without being judiciously selected and mixed. Covers have been made without the underwood necessary for game; and thus in a few years time, after the side branches of the trees have died away, the ground under them has become little better than so much grassland, and the object, for which the cover was made, has almost entirely failed."

To remedy these defects it was Mr. Rawstorne's intention in writing "Gamonia" that those interested in rural pursuits and amusements might profit from his practical experiences in making plantings and covers of value to the planter and sportsman alike.

In developing Battues, extensive preserves and numerous covers were developed for the encouragement of game. He indicates, page 61, that although some soils may be better for breeding than others, there is none but what will "repay the labour and anxiety of the judicious preserver." He points out, page 62, that the nature of the cover and the shelter it affords, are more material than its size. "Trees themselves are of little or no use except for the pheasants to perch in. A strong growth of underwood, with a mixture of briars and brambles that retain their leaves in the winter, is the best calculated for their shelter and protection."

In writing further concerning cover, page 63, he states: "A thick, grassy or sedgy cover may offer an inviting seat to the hare or a resting place to the restless rabbit, but by the pheasant it is considered an insidious receptacle, big with suspicion and alarm. Thus a cover should be rather to a certain degree open, than too close underneath, to give them a greater facility of escape from their active pursuers."

In choosing a location for the development of cover, Rawstorne states, page 64, . . . "there is no better guide than to fix on that which has been already much frequented by game. They know best what suits them." Rather flat ground is recommended over hilly ground and proper preparation of the soil before planting is emphasized.

Among other methods of planting he describes one, page 83, in which the ground should be first trench-plowed very deep in autumn "and laid up in a rough state to receive the amelioration of the winter frosts." The ground is later pulverized by harrowing and, page 84, "laid up into buts of eight yards in width." And, page 84, "The planting may then be begun by a furrow being made in the usual way much after the mode of planting potatoes in drills. Into this furrow are put the trees, and their roots are covered by another turn of the plough, filling up the furrow. If the land is poor and an early rush of wood is desired, manure may be laid in the furrow and the roots of the trees may be put immediately upon it. A man then goes along the furrow that is planted, lifting up the top of the trees and putting them upright, treading down at the same time the soil gently upon the roots. The plough goes on turning over the whole of the land; and when the furrow is come to, which is at a proper distance for another row of trees, the same process is to be pursued as the preceding one . . ."

The land is kept clean by cultivating and hoeing for two or three years, then the "underwood" is put in between the rows which are 5 feet apart.

Mr. Rawstorne concedes, page 87, that "some deserving perhaps the name more of sportsmen than planters may object, because of the delay that is occasioned of two or three years in its providing a cover for game. But let it be considered whether it is not better to make a present and temporary sacrifice, rather than attempt a premature display, which must ultimately fail in its object to a certain extent both as far as pleasure and profit are concerned."

Trees of different types are mixed together, page 92–93, "in such a judicious manner that the ultimate objects of all planting may be best accomplished. It is important to consider the quality of the soil, and likewise to observe what peculiar trees appear to be most indigenous to

it." In selecting species for planting the author points out, page 93, that the planter should "consider what is likely to bring the largest profit at a distant period or within a given time, regard being also had to covers for game."

He indicates that clumps of Scotch firs or larches make ideal winter cover and provide refuge from hunters. The author goes into considerable detail in describing the manner of planting various species of trees. In connection with willows he states, page 104, "Hares and rabbits are extremely partial to a willow bed. Pheasants like one as affording shelter, but it is necessary there should be a plantation close at hand, for them to perch in. A willow bed makes a beautiful shooting cover, when not too thick or too high."

Indicative of Mr. Rawstorne's appreciation of edge effects and connecting travel lanes are these statements found on pages 151 and 152: "Covers in the midst of fields are much the best, not only as affording the greatest variety of food but from the highest and thickest hedges creating shelter, and the roughest cops the most secure breeding grounds. These should be so connected with each other throughout the whole preserves that there should be a chain of them from one extremity to the other."

The author is also cognizant of the excellent game cover provided by plantations of young trees—page 156, "Plantations of young trees will of themselves make the best possible covers for a certain number of years. The side branches lap into each other so that they become an almost impenetrable thicket, sufficiently hollow underneath. Then arrives the critical period, at which they fail. The side branches begin to decay. Everything underneath, even the withering grass has been killed long ago. The owner thinks it right to make up for lost time, and thins largely, believing that this alone will make the cover a good one. O short sighted mortal, why indulge in fanciful and vain expectations? You might almost as well expect to find a pheasant on Salisbury Plain, as in a cover so mutilated and denuded of its richest ornaments."

Commenting on the choice of species for planting as an "underwood" between the principal trees the author states, pages 158–159, "There is nothing to be named with hazel for making a good thick underwood. White or blackthorn is unpleasant to shoot in; and if they get cut down, hares and rabbits are so fond of the young shoots rising from them that it is almost impossible ever to get them up again. As to hollies it is next to a hopeless case to have them where there are rabbits, as they will be sure to bark them the very first severe frost that comes. Hazels bear well the shade; spread out well; and rise quick from the hatchet, when cut down. To give thickness at the bottom, brambles of that sort that keep their leaves in the winter, may be got from the hedgerows, and planted among the hazels. Nothing is so good for ground shooting, as the thick lying they create for hares and rabbits. Privet is good to a certain degree, but it had better only be planted in patches, or at the end for making a thicket to beat out at. It is apt to get laid, and then it prevents their running. It is an advantage to plant box along with it, which keeps it sufficiently open below, but not entirely naked. Gorse makes a good cover for a certain time, and one that all game is fond on, as affording protection both from man and animals: but when very thick, there is no forcing the game out of it; and it soon get hollow, and will not grow for any time under the drip of trees."

Mr. Rawstorne recognized that about every ten years the hazels would reach a height and stage of growth that would not provide optimum cover. He recommended, page 160, that they "should then be cut down quite to the ground, the axe striking upwards, that the boll may not be split." He also recommended, page 160, that each cover development be made sufficiently large so that the underwood could be cut down at four different periods. "There will then be a succession of it at different ages. The younger part will be full of long grass and briars, which will be good for ground shooting: the older more suited to pheasants. A cover thus formed will be good for all sorts of game; and will be always improving instead of deteriorating as is too often the case. It should be kept in mind that trees are of no use except for the pheasants to perch in; and with this in view, too much attention cannot be paid to the underwood which alone constitutes the excellence of a cover."

To keep livestock from ruining a plantation, page 184, the "plashed hedge . . . consisting of

two rows of thorns, one to form the upright stake leaning outwards, the other cut half way through at the bottom, and then intertwined with the upright," is described.

Eggs mowed over in hay making are apparently brought in and incubated under bantam hens and the chicks fed hard boiled eggs and white bread upon hatching. In some instances the young birds are released in cover at about twelve weeks of age; in other instances the birds are field-reared according to the system currently being tested in Oregon. Barley in the sheaf and potatoes either raw or steamed, are listed among the pheasant foods.

Concerning damage and control measures Rawstorne states, page 196, "There is one expense in preserving game which, though it comes in an indirect manner . . . is a never failing evil to the farmer . . . First when the land is sown it becomes necessary that an extra quantity of seed should be used in consequence of the game. Some imagine that they will only take that which lies on the surface, and which is chiefly the light grain: but can it be supposed that they will give the preference to that, or that they cannot discriminate betwixt the two."

And on page 197—"Pheasants also do much injury to a potato crop before it is got up. The time when this becomes nearly ripe, is when every pheasant will be, if possible, in a potato field."

He comments on the much greater destructiveness of rabbits however, and on page 200 states: "A large stock of pheasants can only be raised by skill and management; but as to rabbits there requires little more than permission given to increase and multiply."

Some of the author's observations on land use as affecting game populations are of interest. Thus, on page 204, he states, "Norfolk stands pre-eminent as the county for game . . . It is in general thinly inhabited, consists of a light sandy soil, favorable for breeding, and possesses large fields of turnips. It is the extent of these that makes it so superior for partridge shooting." And on pages 206 and 207: "One great cause of populous districts being adverse to game is that most of the land is in grass. Where there is a great demand for milk and butter, the most profitable farming is that which can give a ready supply of these necessary articles . . . In a country so circumstanced it would be an absurdity to suppose that it could compete in quantity of game with one better adapted for it, perhaps from quality of soil as well as the mode of cultivation . . . A whole district in tillage with a mixture of corn, clover, and potatoes, makes just as good shooting ground as can exist anywhere. Potatoes are excellent for sport, whilst they last; and come very near to turnips."

Concerning game violations he states, page 208, "There must always be some notoriously bad characters, who will not get a livelihood by honest means. These perhaps begin by first snaring a hare . . . They advance on by degrees in taking a chance shot at night when they think the keepers are off their posts." He points out also, page 209, that damage is done by accidental trespassers and by house cats.

These excerpts illustrate sufficiently well, I believe, the interesting manner in which the book is written. They show also that many of the ideas and principles of game management which we are prone to consider modern and enlightened, were known well over 100 years ago. How many of the present day wildlife plantings are failures because of the lack of care given to them—the same carelessness in preparation of the planting site and later cultivation decried by author Rawstorne! How familiar his observations on game law violators, mowing losses of pheasants, edge effects, travel lanes, stages of plant succession in relation to game, cover management by cutting on a rotation basis, game damage to crops, land use-wildlife relationships and multiple use!

It is highly probable that many modern game managers could learn some "tricks of the trade" by a careful reading of "Gamonia." For example, although treated but briefly, the methods described for developing hedgerows in England might be used to good advantage in some of our field border plantings.

Adding to the general interest of the book and to its value from a collector's standpoint are 15 delicately colored drawings of hunting scenes by T. J. Rawlings.—DANIEL L. LEEDY.